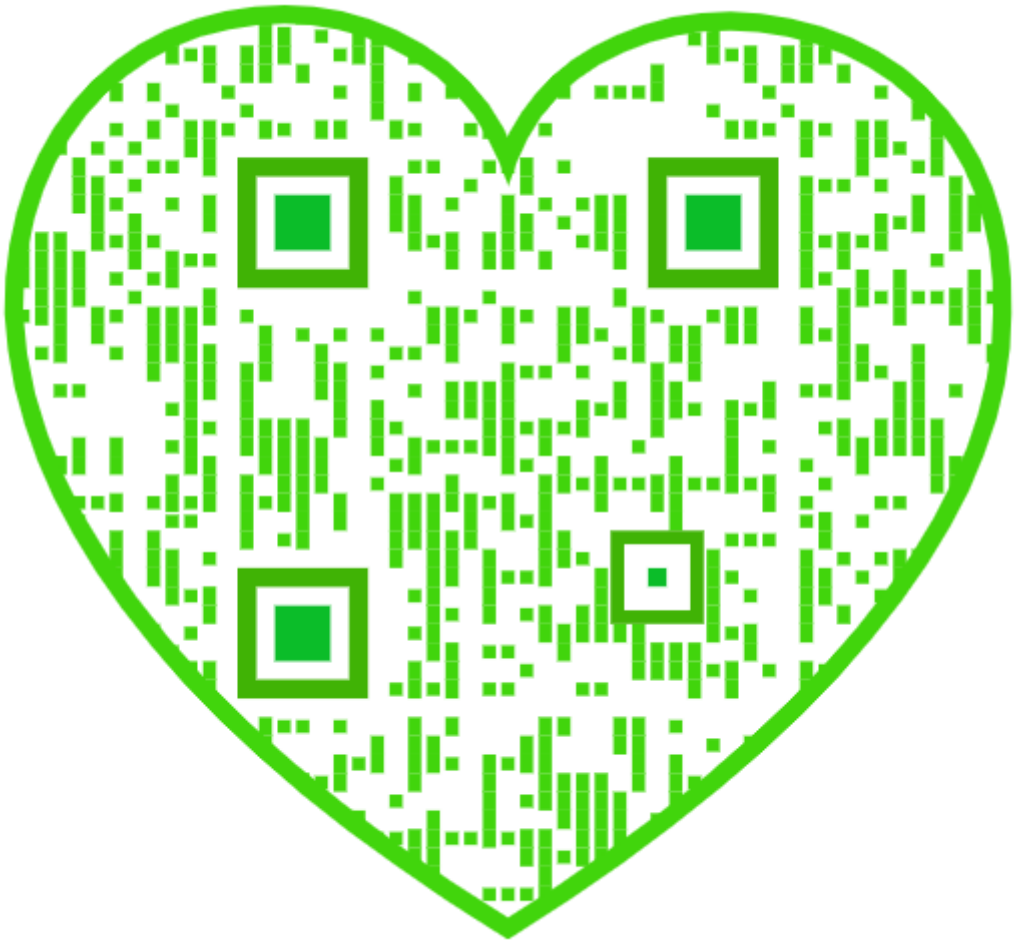

TRANSACTIONS

OF THE

AMERICAN ASSOCIATION

OF

OBSTETRICIANS AND GYNECOLOGISTS.

VOL. XII.

FOR THE YEAR 1899.

PHILADELPHIA:
WM. J. DORNAN, PRINTER.
1900.

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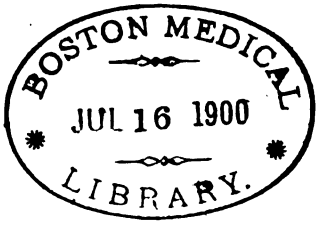

NOTE.

THE Association does not hold itself responsible for the views enunciated in the papers and discussions published in this volume.

WILLIAM WARREN POTTER, *Secretary*,
284 FRANKLIN STREET, BUFFALO.

[MINUTES and discussions stenographically reported by WILLIAM WHITFORD, Chicago, Ill.]

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CONSTITUTION AND BY-LAWS

OF THE

AMERICAN ASSOCIATION

OF

OBSTETRICIANS AND GYNECOLOGISTS,

TOGETHER WITH

MINUTES OF THE TWELFTH ANNUAL MEETING.

AMERICAN ASSOCIATION

OF

OBSTETRICIANS AND GYNECOLOGISTS.

CONSTITUTION.

I. The name of this Association shall be THE AMERICAN ASSOCIATION OF OBSTETRICIANS AND GYNECOLOGISTS.

II. Its object shall be the cultivation and promotion of knowledge in whatever relates to Abdominal Surgery, Obstetrics, and Gynecology.

MEMBERS.

III. The members of this Association shall consist of Ordinary Fellows, Honorary Fellows, and Corresponding Fellows.

The Ordinary Fellows shall not exceed one hundred and twenty-five in number.

The Honorary Fellows shall not exceed ten American and twenty-five foreign.

Candidates shall be proposed to the Executive Council at least one month before the first day of meeting by two Fellows, and shall be balloted for at the annual meeting, a list of names having been sent to every Fellow with the notification of the meeting.

A two-thirds vote in the affirmative of all the members present shall be necessary to elect—fifteen Fellows at least being in attendance.

All candidates for active fellowship shall submit to the Executive Council, at least one month before the annual meeting, an original paper relating to Abdominal Surgery, Obstetrics, or Gynecology.

HONORARY FELLOWS.

IV. The power of nominating Honorary Fellows shall be vested in the Executive Council.

Their election shall take place in the same manner as that of Ordinary Fellows.

They shall enjoy all the privileges of Ordinary Fellows, excepting to vote or hold office, but shall not be required to pay any fee.

CORRESPONDING FELLOWS.

V. The Corresponding Fellows shall be recommended by the Executive Council and elected by the Association.

They shall enjoy all the privileges of Ordinary Fellows, excepting to vote or hold office, and shall be entitled to a copy of the annual *Transactions*.

They shall pay an annual fee of five dollars.

OFFICERS.

VI. The officers of this Association shall be a President, two Vice-presidents, a Secretary, a Treasurer, and six Executive Councillors.

The nomination of all officers shall be made in open session at the business meeting, and the election shall be by ballot.

The first five officers shall enter upon their duties immediately before the adjournment of the meeting at which they shall be elected, and shall hold office for one year.

["At the election next succeeding the adoption of these laws, the full number of Executive Councillors shall be elected; two for a term of three years, two for a term of two years, and two for a term of one year.

"At every subsequent election two Councillors shall be elected for a term of three years, and shall continue in office until their successors shall have been elected and shall have qualified."][1]

Any vacancy occurring during the recess may be filled temporarily by the Executive Council.

ANNUAL MEETINGS.

VII. The time and place of holding the annual meeting shall be determined by the Association or may be committed to the Executive Council each time before adjournment.

It shall continue for three days, unless otherwise ordered by vote of the Association.

AMENDMENTS.

VIII. This Constitution may be amended by a two-thirds vote of all the Fellows present at the annual meeting: *provided*, that notice of the proposed amendment shall have been given in writing at the annual meeting next preceding: and *provided, further*, that such notice shall have been printed in the notification of the meeting at which the vote is to be taken.

[1] Amendment adopted September 21, 1898.

AMERICAN ASSOCIATION

OF

OBSTETRICIANS AND GYNECOLOGISTS.

BY-LAWS.

THE PRESIDING OFFICER.

I. The President, or in his absence one of the Vice-presidents, shall preside at all meetings, and perform such other duties as ordinarily pertain to the Chair.

The presiding officer shall be *ex-officio* chairman of the Executive Council, but shall vote therein only in case of a tie.

SECRETARY.

II. The Secretary shall attend and keep a record of all meetings of the Association and of the Executive Council, of which latter he shall be *ex-officio* clerk, and shall be entitled to vote therein.

He shall collect all moneys due from the members, and shall pay the same over to the Treasurer, taking his receipt therefor.

He shall supervise and conduct all correspondence of the Association; he shall superintend the publication of the *Transactions* under the direction of the Executive Council, and shall perform all the ordinary duties of his office.

He shall be the custodian of the seal, books, and records of the Association.

TREASURER.

III. The Treasurer shall receive all moneys from the Secretary, pay all bills, and render an account thereof at the annual meetings, when an Auditing Committee shall be appointed to examine his accounts and vouchers.

EXECUTIVE COUNCIL.

IV. The Executive Council shall meet as often as the interests of the Association may require. The President, or any three members, may call a meeting, and a majority shall constitute a quorum.

It shall have the management of the affairs of the Association, subject to the action of the house at its annual meetings.

It shall have control of the publications of the Association, with full power to accept or reject papers or discussions.

It shall have control of the arrangements for the annual meetings, and shall determine the order of the reading of papers.

It shall constitute a court of inquiry for the investigation of all charges against members for offences involving law or honor; and it shall have the sole power of moving the expulsion of any Fellow.

ORDER OF BUSINESS.

V. The Order of Business at the annual meetings of the Association shall be as follows:

1. General meeting at 10 o'clock A.M.
   a. Reports of Committees on Scientific Questions.
   b. Reading of Papers and Discussion of the same.
2. One Business Meeting shall be held at half-past nine o'clock A.M. on the first day of the session, and another on the evening of the second day (unless otherwise ordered by vote), at which only the Fellows of the Association shall be present. At these meetings the Secretary's Record shall be read; the Treasurer's Accounts submitted; the Reports of Committees on other than scientific subjects offered; and all Miscellaneous Business transacted.

PAPERS.

VI. The titles of all papers to be read at any annual meeting shall be furnished to the Secretary *not later* than one month before the first day of the meeting.

No paper shall be read before the Association that has already been published, or that has been read before any other body.

Not more than thirty minutes shall be occupied in reading any paper before the Association.

Abstracts of all papers read should be furnished to the Secretary at the meeting.

All papers read before the Association shall become its sole property if accepted for publication; and the Executive Council may decline

to publish any paper not handed to the Secretary *complete* before the final adjournment of the annual meeting.

QUORUM.

VII. The Fellows present shall constitute a quorum for all business, excepting the admission of new Fellows or acting upon amendments to the Constitution, when not less than fifteen Fellows must be present.

DECORUM.

VIII. No remarks reflecting upon the personal or professional character of any Fellow shall be in order at any meeting, except when introduced by the Executive Council.

FINANCE.

IX. Each Fellow on admission shall pay an initiation fee of twenty-five dollars, which shall include his dues for the first year.

Every Fellow shall pay *in advance* (*i. e.*, at the beginning of each fiscal year) the sum of twenty dollars annually thereafter.

[A fiscal year includes the period of time between the first day of one annual meeting and the first day of the next.]

Any Fellow neglecting to pay his annual dues for two years may forfeit his membership, upon vote of the Executive Council.

The Secretary shall receive annually a draft from the President drawn on the Treasurer for a sum, to be fixed by the Executive Council, for the services he shall have rendered the Association during the year.

A contingent fund of one hundred dollars shall be placed annually at the disposal of the Secretary for current expenses, to be disbursed by him, and for which he shall present proper vouchers.

ATTENDANCE.

X. Any Fellow who shall neither attend nor present a paper for three consecutive years, unless he offer a satisfactory excuse, may be dropped from fellowship upon vote of the Executive Council.

RULES.

XI. *Roberts' Rules of Order* shall be accepted as a parliamentary guide in the deliberations of the Association.

AMENDMENTS.

XII. These By-laws may be amended by a two-thirds vote of the Fellows present at any meeting; *provided*, previous notice in writing shall have been given at the annual meeting next preceding the one at which the vote is to be taken.

OFFICERS FOR 1899-1900.

PRESIDENT.

RUFUS BARTLETT HALL, CINCINNATI.

VICE-PRESIDENTS.

LEHMAN HERBERT DUNNING, INDIANAPOLIS.
THOMAS JEFFERSON CROFFORD, MEMPHIS.

SECRETARY.

WILLIAM WARREN POTTER, BUFFALO.

TREASURER.

XAVIER OSWALD WERDER, PITTSBURG.

EXECUTIVE COUNCIL.

WILLIAM E. B. DAVIS, BIRMINGHAM.
JOHN MILTON DUFF, PITTSBURG.
LEHMAN HERBERT DUNNING, INDIANAPOLIS.
WALTER BENAJAH CHASE, NEW YORK.
ALBERT VANDER VEER, ALBANY.
LEWIS SAMUEL McMURTRY, LOUISVILLE.

HONORARY FELLOWS.

* Deceased.

1889.—BANTOCK, GEORGE GRANVILLE, M.D., F.R.C.S. Ed. Surgeon to the Samaritan Free Hospital. 12 Granville Place, Portman Square, W., London, England.

1899.—BALLANTYNE, JOHN WILLIAM, M.D., F.R.C.P.E., F.R.S. Edin. Lecturer on Midwifery and Gynecology, School of the Royal Colleges, Edinburgh; Examiner in Midwifery and Gynecology in the University of Aberdeen; Vice-President of the Edinburgh Obstetrical Society; Honorary Fellow of the Glasgow Obstetrical and Gynecological Society. 24 Melville Street, Edinburgh, Scotland.

1889.—BARBOUR, A. H. FREELAND, M.A., B.S.C., M.D., F.R.C.P. Ed., F.R.S. Ed. Lecturer on Midwifery and Diseases of Women in the Edinburgh Medical School; Assistant Physician to the Royal Maternity Hospital; Assistant Physician for Diseases of Women to the Royal Infirmary; Physician to the Women's Dispensary; Fellow of the Edinburgh and London Obstetrical Societies and of the British Gynecological Society; Corresponding Fellow of the Royal Academy of Medicine, Turin. 4 Charlotte Square, Edinburgh, Scotland.

1892.—*BOISLINIÈRE, L. CH., A.B , M.D., LL.D. 1896.

1890.—CHAMPIONNIÈRE, JUST. LUCAS, M.D. 3 Avenue Montaigne, Paris, France.

1889.—*CHARPENTIER, ARTHUR LOUIS ALPHONSE, M.D. 66 Rue de Miromesnil, Paris, France. 1899.

1888.—CORDES, AUGUST ELISÉE, M.D. Member of the Royal College of Physicians, London; Fellow of the Obstetrical Society of London and of the British Gynecological Society; Corresponding National Member of the Obstetrical and Gynecological Society of Paris; Honorary Fellow of the Detroit Gynecological Society; late "Chirurgien-adjoint" of the Obstetrical and Gynecological Clinic at the Maternity at Geneva; Consulting Accoucheur of the Miséricorde Hospital, etc.; Perpetual Member of the Société Obstétricale de France, Paris, France. 12 Rue Bellot, Geneva, Switzerland.

1890.—*Corson, Hiram, M.D. 1896.

1889.—Croom, J. Halliday, M.D., F.R.C.P.E., F.R.C.S.E., F.R.S.E. Physician to and Clinical Lecturer on Diseases of Women, Royal Infirmary, Edinburgh; Physician to the Royal Maternity Hospital; Lecturer on Midwifery and the Diseases of Women at the School of Medicine; Consulting Physician for Diseases of Women, Western Dispensary. 25 Charlotte Square, Edinburgh, Scotland.

1889.—*Dunlap, Alexander, A.M., M.D. Springfield, O. 1894.

1888.—*Edis, Arthur Wellesley, M.D. Lond., F.R.C.S., M.R.S.C.S. London, England. 1893.

1889.—*Eklund, Abraham Fredrik, M.D. 3 A. Sibyllegatan, Stockholm, Sweden. 1898.

1891.—Fernandez, Juan Santos, M.D. Calle de la Reina, No. 92, Havana, Cuba.

1891.—*Fisher, George Jackson, A.M., M.D. Sing Sing, N. Y. 1893.

1889.—Freund, William Alexander, M.D. Professor and Director of the Clinic for Diseases of Women in the Emperor William University. 2 Nikolaustaden, Strassburg, Germany.

1896.—Gaston, James McFadden, A.M., M.D. Professor of Surgery in the Southern Medical College, Atlanta; Fellow of the American Surgical Association; Member of the Southern Surgical and Gynecological Association. 421 Capitol Avenue, Atlanta, Ga.

1892.—*Green, Traill, M.D., LL.D. Easton, Pa., 1897.

1894.—Jacobs, Charles, M.D. Professor of the Faculty of Medicine of Brussels; Secretary-General of the Permanent Committee of the Periodic International Congress of Gynecology and Obstetrics; Honorary President of the Belgian Society of Gynecology and Obstetrics; Honorary Fellow of the Gynecological Societies of New York and Chicago; Member of the Southern Surgical and Gynecological Association; Corresponding Member of the Gynecological Society of Paris; Surgeon to the Brussels Polyclinic. 53 Boulevard de Waterloo, Brussels, Belgium.

1889.—*Keith, Thomas, M.D. London, Eng., 1896.

1889.—Leopold, G., M.D. Professor in the Royal Clinic for Diseases of Women. 12 Seminar-Strasse, Dresden, Germany.

1894.—Maclean, Donald, M.D. President of the American Medical Association 1894. 72 Lafayette Avenue, Detroit, Mich.

1890.—MARTIN, AUGUST, M.D. Professor of Gynecology in the University of Griefswald, Griefswald, Germany.

1895.—*MASTIN, CLAUDIUS HENRY, M.D., LL.D. Mobile, Ala., 1898.

1897.—MATHEWS, JOSEPH MCDOWELL, M.D. Professor of Diseases of the Rectum and Clinical Surgery, Hospital College of Medicine; President of the Kentucky State Board of Health; First Vice-President American Medical Association 1898; President, 1899. 923 Fourth Avenue, Louisville, Kentucky.

1891.—MOSES, GRATZ ASHE, M.D. Emeritus Professor of Obstetrics and Gynecology in the Missouri Medical College; formerly in charge of the Woman's Department, St. Louis Hospital, and Clinical Lecturer in Gynecology, St. Louis Medical College; Physician to Augusta Hospital. 3941 West Bell Place, St. Louis, Mo.

1889.—NICOLAYSEN, JULIUS, M.D. Professor of Surgery in the University of Norway. Christiania, Norway.

1891.—PIETRANERA, E., M.D. Professor of Obstetrics in the Medical Department of the National University; Director of the Maternity Branch of the Clinical Hospital. 2711 Calle Rio Adaria, Buenos Ayres, Argentine Republic, S. A.

1889.—SAENGER, MAX, M.D. Professor of Obstetrics and Gynecology, University of Prague; late President of the Leipzig Obstetrical Society; Honorary Member of the Obstetrical Societies of Philadelphia and Chicago; Honorary Member of the Belgian Gynecological Society, Brussels; Honorary Member of the Gynecological Society, Kiew; Corresponding Member of the Medical Society of Christiania, Norway. Heuwaagsplatz 3, Prague, Germany.

1890.—SAVAGE, THOMAS, M.D., F.R.C.S. Eng. Surgeon to the Birmingham Hospital for Women. 33 Newhall Street, Birmingham, England.

1889.—SCHULTZE, BERNHARD SIGMUND, M.D. Professor of Gynecology; Director of the Lying-in Institute and of the Gynecological Clinic. 2 Sellierstrasse, Jena, Germany.

1899.—SINCLAIR, WILLIAM JAPP, M.A., M.D. (Aberd.), M.R.C.P. Professor of Obstetrics and Gynecology, Owens College, Victoria University; Physician Manchester Southern Hospital for Diseases of Women and Children. 250 Oxford Road, Manchester, England.

1896.—SEGOND, PAUL, M.D. Professor of the Faculty of Medicine. Paris; Surgeon to the Salpêtrière; Principal Physician to the Orleans Railroad. 11 Quai d'Orsay, Paris.

1894.—*Slaviansky, Kronid, M.D. 24 Liteinaia Street, St. Petersburg, Russia. 1898.

1888.—*Smith, J. Greig, M.A., C.M., M.B., F.R.S.E. Bristol, England. 1897.

1899.—Storrs, Melancthon, A.M., M.D. Attending Surgeon to the Hartford Hospital, and President of Connecticut State Medical Society. (Transferred from Ordinary Fellow.) 91 Ann Street, Hartford, Conn.

1896.—Sternberg, George Miller, A.M., M.D., LL.D. Brigadier-General and Surgeon-General, U. S. Army. Washington, D. C.

1888.—*Tait, Lawson, M.D., LL.D., F.R.C.S.E. Birmingham, England. 1899.

1888.—Williams, Sir John, Bart., M.D., F.R.C.P. 63 Brook Street, Grosvenor Square, W., London, England.

1889.—v. Winckel, F., M.D. Professor of Gynecology and Director of the Royal Hospital for Women; Member of the Supreme Council and of the Faculty of Medicine in the University of Munich. 16a Sonnenstrasse, Munich, Germany.

Total, twenty-six Honorary Fellows.

CORRESPONDING FELLOWS.

1899.—BEUTTNER, OSCAR, M.D., Geneva, Switzerland.

1891.—GRIFFIN, HERBERT SPOHN, B.A., M.D. Surgeon to Hamilton City Hospital; Examiner in Obstetrics, University of Toronto. 157 Main Street, Hamilton, Ontario, Canada.

1891.—MACHELL, HENRY THOMAS, M.D., L.R.C.P. Ed. Lecturer on Obstetrics, Woman's Medical College; Surgeon to St. John's Hospital for Women; Physician to Victoria Hospital for Sick Children and to Hillcrest Convalescent Home. 95 Bellevue Avenue, Toronto, Ontario, Canada.

1898.— WRIGHT, ADAM HENRY, B.A., M.D., Univ. Toronto, M.R.C.S. Eng. Professor of Obstetrics in the University of Toronto; Obstetrician and Gynecologist to the Toronto General Hospital and Burnside Lying-in Hospital. President, 1891. 30 Gerrard Street, East, Toronto, Ont., Canada.

Total, four Corresponding Fellows.

ORDINARY FELLOWS.

* Deceased. † Resigned.

1890.—ASDALE, WILLIAM JAMES, M.D. Professor of Diseases of Women, Western Pennsylvania Medical College, Medical Department University of Western Pennsylvania. 5523 Ellsworth Avenue, Pittsburg, Pa.

1895.—BACON, JOSEPH BARNES, M.D. Professor of Rectal Diseases at the Post-Graduate Medical School; Instructor in Clinical Surgery in the Medical Department of Northwestern University. 4125 Drexel Boulevard, Chicago, Ill.

Founder.—BAKER, WASHINGTON HOPKINS, M.D. Senior Obstetrician to the Maternity Hospital; Physician to the German Hospital. 1610 Summer Street, Philadelphia, Pa.

1895.—BALDWIN, JAMES FAIRCHILD, A.M., M.D. Chancellor of and Professor of Surgical Gynecology in the Ohio Medical University; Gynecologist at the Protestant Hospital. 112 North Fourth Street, Columbus, O.

1889.—BARROW, DAVID, M.D. Member of the Southern Surgical and Gynecological Association. 22 East Market Street, Lexington, Kentucky.

1892.—BLUME, FREDERICK, M.D. Gynecologist to the Allegheny General Hospital and Pittsburg Free Dispensary; Obstetrician to the Roselia Maternity Hospital; Consulting Gynecologist to the Mercy Hospital; President of the Pittsburg Obstetrical Society, 1892. 524 Penn Avenue, Pittsburg, Pa.

1896.—BOSHER, LEWIS C., M.D. Professor of the Principles of Surgery and Clinical Lecturer on Genito-urinary Surgery, Medical College of Virginia; Visiting Surgeon to the Old Dominion Hospital. 717 East Franklin Street, Richmond, Va.

Founder.—BOYD, JAMES PETER, A.M., M.D. Professor of Obstetrics, Gynecology, and Diseases of Children in the Albany Medical College; Gynecologist to the Albany Hospital; Consulting Obstetric Surgeon to St. Peter's Hospital; Fellow of the British Gynecological Society. 152 Washington Avenue, Albany, N. Y.

1889.—BRANHAM, JOSEPH H., M.D. Demonstrator of Anatomy in the College of Physicians and Surgeons; Visiting Surgeon to Bayview Hospital. 538 North Arlington Avenue, Baltimore, Md.

1894.—BROWN, JOHN YOUNG, JR., M.D. Late First Assistant Physician in the Central Kentucky Asylum for the Insane; President Mississippi Valley Medical Association, 1898. 4236 Morgan Street, St. Louis, Mo.

1889.—*BURNS, BERNARD, M.D., Allegheny, Pa. 1892.

1898.—CAMERON, MARKLEY CONNELL, M.D. Demonstrator of Gynecology, Western Pennsylvania Medical College; Assistant Physician, Western Pennsylvania Hospital. 178 Forty-third Street, Pittsburg, Pa.

Founder.—CARSTENS, J. HENRY, M.D. Professor of Obstetrics and Clinical Gynecology in the Detroit College of Medicine; Gynecologist to the Harper Hospital; Attending Physician to the Woman's Hospital; Obstetrician to the House of Providence; President of the Detroit Gynecological Society, 1892. *Vice-President*, 1888–89; *President*, 1895; *Executive Council*, 1896–98. 620 Woodward Avenue, Detroit, Mich.

1895.—CHASE, WALTER BENAJAH, M.D. Gynecologist to the Bushwick Hospital; Attending Surgeon and Gynecologist, Central Hospital and Dispensary; Consulting Gynecologist to the Long Island College Hospital; Councillor to the Long Island College Hospital; Fellow of the Brooklyn Gynecological Society (President, 1893); Member Medical Society County of Kings (President, 1892); Permanent Member Medical Society State of New York; Member of the Brooklyn Pathological Society, and Honorary Member of the Queens County Medical Society. *Executive Council*, 1899–1900. 263 Hancock Street, New York, Borough of Brooklyn.

Founder.—CLARKE, AUGUSTUS PECK, A.M., M.D. Dean and Professor of Gynecology and Abdominal Surgery in the College of Physicians and Surgeons, Boston; Vice-President of the American Medical Association, 1896; President of the Gynecological Society of Boston, 1891–92; Vice-President of the Pan-American Medical Congress, 1893, and of the Pan-American Medical Congress, Mexico, 1896. Honorary President of the Section of Obstetrics and Gynecology of the Twelfth International Medical Congress, Moscow, Russia, 1897; Member of the Massachusetts Medical Society; Fellow of the American Academy of Medicine; Member of the American Public Health Association. 825 Massachusetts Avenue, Cambridge, Mass.

1890.—*COLES, WALTER, M.D., St. Louis, Mo. 1892.

1892.—Cordier, Albert Hawes, M.D. Professor of Abdominal Surgery in the Kansas City Medical College; First Vice-President of the Kansas State Medical Society, 1892; Corresponding Member of the Philadelphia Obstetrical Society. *Vice-President*, 1896. 310 Rialto Building, Kansas City, Mo.

1894.—Crofford, Thomas Jefferson, M.D. Professor of Physiology and Clinical Lecturer on Diseases of Women in the Memphis Hospital Medical College; Member of the Southern Surgical and Gynecological Association. *Vice-President*, 1900. 155 Third Street, Memphis, Tenn.

1897.—Cumston, Charles Greene, B.M.S., M.D. (Geneva, Switzerland). Assistant Professor of Surgical Pathology, Tuft's College Medical School, Boston; Member of the Massachusetts Medical Society; Honorary Member of the Surgical Society of Belgium, and Corresponding Member of the Obstetrical and Gynecological Society of Paris; Corresponding Member of the Association of Genito-urinary Surgeons of France; Corresponding Member of the Pathological Society of Brussels, Belgium; Corresponding Member of the Electrotherapeutical Society of France. 871 Beacon Street, Boston, Mass.

Founder.—Cushing. Clinton, M.D. Professor of Gynecology in the Cooper Medical College; Consulting Surgeon to the French Hospital. *Executive Council*, 1888–1891. 1607 I Street, N.W., Washington, D. C.

1892.—Cushing, Ernest Watson, A.B., M.D. Surgeon of the Woman's Charity Club Hospital; Editor of the *Annals of Gynecology and Pediatry*. 168 Newbury Street, Boston, Mass.

1894.—Davega, S. M., M.D. Surgeon to the Richmond and Danville Railroad, C. and L. R. R., C. and C. R. R., G. C. and N. R. R. Wylie Street, Chester, S. C.

1889.—Davis, William Elias B., M.D. Professor of Gynecology and Abdominal Surgery in the Birmingham Medical College; Secretary of the Southern Surgical and Gynecological Association, 1888–1900; formerly Surgeon to the Birmingham Hospital of United Charities; President of the Tri-state Medical Society of Alabama, Georgia, and Tennessee, 1892; Secretary of the Surgical Section of the American Medical Association, 1891; Honorary President of the Section on Gynecology and Abdominal Surgery of the First Pan-American Medical Congress; Honorary Member of the Medical Society of the State of New York. *Vice-President*, 1895. *Executive Council*, 1897–1900. 2031 Avenue G, Birmingham, Ala.

1896.—Deaver, John Blair, M.D. Formerly Assistant Professor of Applied Anatomy at the University of Pennsylvania;

Visiting Surgeon to the German Hospital. 1634 Walnut Street, Philadelphia, Pa.

1892.—DORSETT, WALTER BLACKBURN, M.D. Professor of Obstetrics in the Beaumont Hospital Medical College; President of the St. Louis Medical Society, 1892. President of the Missouri State Medical Society, 1900. *Vice-President*, 1898. 3941 West Bell Place, St. Louis, Mo.

1889.—DOUGLAS, RICHARD, M.D. Professor of Gynecology and Abdominal Surgery in the Vanderbilt Medical College; President of the Tri-State Medical Society of Alabama, Georgia, and Tennessee, 1893; Fellow of the British Gynecological Society; President of the Southern Surgical and Gynecological Association, 1898. *Vice President*, 1898. 110 Spruce Street, Nashville, Tenn.

1892.—DUFF, JOHN MILTON, A.M., M.D., Ph.D. Chairman of the Section on Obstetrics and Diseases of Women in the American Medical Association, 1893; Professor of Obstetrics in the Western Pennsylvania Medical College; Gynecologist to the Western Pennsylvania Hospital; Consulting Surgeon and Gynecologist to the South Side Hospital; Fellow of the American Academy of Medicine; President of the Pittsburg Obstetrical Society, 1891. *Executive Council*, 1898–1900. 2006 Carson Street, Pittsburg, Pa.

1898.—DUNN, JAMES C., M.D. Obstetrician to Reineman Maternity Hospital. 208 Winebiddle Avenue, Pittsburg, Pa.

1895.—†DUNN, JAMES HENRY, M.D. Minneapolis, Minn., 1899.

1895.—DUNN, B. SHERWOOD-, M.D. Officier d'Académie; Corresponding Member of the Société Obstétrique et Gynécologique de Paris; Member of the Société Clinique des Praticiens de France, etc. Room 159, 80 Broadway, New York.

1892.—DUNNING, LEHMAN HERBERT, M.D. Professor of Diseases of Women in the Medical College of Indiana; Consulting Gynecologist to the Indianapolis City Hospital and Dispensary. *Executive Council*, 1899–1900. *Vice-President*, 1900. 431 North Alabama Street, Indianapolis, Ind.

1895.—EARLE, FRANK BRECKINRIDGE, M.D. Professor of Obstetrics at the College of Physicians and Surgeons. 903 W. Monroe Street, Chicago, Ill.

1899.—EASTMAN, THOMAS BOOKER, A.B., M.D. Professor of the Medical and Surgical Diseases of Women, Central College of Physicians and Surgeons; Gynecologist to the City Hospital, City Dispensary, and Central Free Dispensary. 331 North Delaware Street, Indianapolis, Ind.

1895.—FERGUSON, ALEXANDER HUGH, M.D. Professor of Surgery at the Chicago Post-Graduate Medical School. 2950 Indiana Avenue, Chicago, Ill.

1895.—FISH, EDMUND FROST, M.D. Professor of Gynecology in the Milwaukee Medical College; Gynecologist to the Trinity, St. Joseph's, and Milwaukee City Hospitals; Gynecologist to the Milwaukee Free Dispensary. 211 Grand Avenue, Milwaukee, Wis.

1890.—FREDERICK CARLTON CASSIUS, B.S., M.D. Clinical Professor of Gynecology in the Medical Department of Buffalo University; Obstetrician and Gynecologist to the Buffalo Woman's Hospital; Obstetrician to the Widows' and Infants' Asylum; Gynecologist to the Erie County Hospital. 64 Richmond Avenue, Buffalo, N. Y.

1891.—GIBBONS, HENRY, JR., A.M., M.D. Dean and Professor of Obstetrics and Diseases of Women and Children in Cooper Medical College; Consulting Physician to the French and the Children's Hospitals. 920 Polk Street, San Francisco, Cal.

1895.—GILLIAM, DAVID TOD, M.D. Professor of Gynecology, Starling Medical College; Gynecologist to St. Anthony Hospital; Gynecologist to St. Francis Hospital; Consulting Gynecologist to State Street Dispensary; Member of the American Medical Association, Mississippi Valley Medical Association; Ohio State Medical Society; Honorary Member of the Northwestern Medical Society; Member and Ex-President of Columbus Academy of Medicine. 70 Winner Avenue, Columbus, O.

1895.—GOLDSPOHN, ALBERT, M.D. Professor of Gynecology, Post-Graduate Medical School; Senior Gynecologist, German Hospital; Attending Gynecologist, Post-Graduate and Charity Hospitals. 519 Cleveland Avenue, Chicago, Ill.

1894.—GRIFFITH, JEFFERSON DAVIS, M.D. Professor of Surgery in the Kansas City Medical College; Surgeon to St. Joseph's Hospital and to the Children's Hospital. Corner Grand Avenue and Thirty-fifth Street, Kansas City, Mo.

1892.—HAGGARD, WILLIAM DAVID, M.D. Professor of Gynecology and Diseases of Children in the Medical Department of the University of Tennessee; Consulting Gynecologist to the City Hospital; Gynecologist to St. Margaret's Hospital; Chairman of Section on Diseases of Children, American Medical Association, 1886; Fellow (President, 1888) of the Southern Surgical and Gynecological Association; President of the Nashville Academy of Medicine and Surgery. 312 North High Street, Nashville, Tenn.

1889.—HALL, RUFUS BARTLETT, A.M., M.D. Professor of Clinical Gynecology at the Miami Medical College; Gynecologist to the Presbyterian Hospital; Member of the British Gynecological Association; of the Southern Surgical and Gynecological Association; of the American Medical Association; of the Ohio State Medical Society (President, 1900); of the Cincinnati Academy of Medicine; President of the Cincinnati Obstetrical Society, 1896. *Vice-President*, 1891. *President*, 1900. Berkshire Building, 628 Elm Street, Cincinnati, Ohio.

1894.—HAYD, HERMAN EMILIE, M.D., M.R.C.S., Eng. Gynecologist to the Erie County Hospital. 493 Delaware Avenue, Buffalo, N. Y.

Founder.—*HILL, HAMPTON EUGENE, M.D. 1894.

1891.—HOLMES, JOSUS BILLINGTON SANDERS, M.D. Professor of Obstetrics in the Southern Medical College; President of the Georgia State Medical Association, 1890; Member of the Southern Surgical and Gynecological Association; Member of the American Medical Association. 17 West Cain Street, Atlanta, Georgia.

1891.—HOWITT, HENRY, M.D., M.R.C.S., Eng. Surgeon to the Guelph General and St. Joseph's Hospital, Guelph; Member of the British and Ontario Medical Association; Medical Health Officer for the City of Guelph. *Vice President*, 1895. 235 Woolwich Street, Guelph, Ontario, Canada.

1896.—HUGHES, GEORGE MAURICE, M.D. Physician in Charge of the Obstetric and Gynecological Department of the Philadelphia Dispensary. 241 North Eighteenth Street, Philadelphia, Pa.

1895.—HUMISTON, WILLIAM HENRY, M.D. Clinical Lecturer on Gynecology at the Medical Department of the Western Reserve University; Consulting Gynecologist to the City Hospital. 122 Euclid Avenue, Cleveland, O.

1898.—HYDE, JOEL W., M.D. Obstetric Surgeon to St. Mary's Hospital; Consulting Obstetrician to the Long Island College Hospital; Consulting Gynecologist to Central Hospital. 215 Schermerhorn Street, Brooklyn, N. Y.

1892.—HYPES, BENJAMIN MURRAY, A.M., M.D., Professor of Obstetrics in the Marion-Sims Medical College. 2005 Victor Street, St. Louis, Mo.

Founder.—ILL, EDWARD JOSEPH, M.D. Surgeon to the Woman's Hospital; Gynecologist to St. Barnabas's and Consulting Gyn ecologist to the German Hospitals. *Vice-President*, 1893; *President*, 1899. 1002 Broad Street, Newark, N. J.

1897.—INGRAHAM, HENRY DOWNER, M.D. Clinical Professor of Gynecology and Pediatrics, Medical Department of the University of

Buffalo; Consulting Gynecologist to the Buffalo Woman's Hospital, and to the Erie County Hospital; Consulting Gynecologist to Providence Hospital. 405 Franklin Street, Buffalo, N. Y.

Founder.—†JARVIS, GEORGE CYPRIAN, M.D. Hartford, Conn. 1900.

1894.—JAYNE, WALTER ADDISON, M.D. Professor of Gynecology in the Medical Department of the University of Denver; Consultant in Gynecology, St. Luke's Hospital; Gynecologist to the Arapahoe County Hospital, Denver. 217 McPhee Building, Denver, Col.

1892.—JELKS, JAMES THOMAS, M.D.. President of the Arkansas Medical Society, 1892; Chairman of the Section of Surgery in the American Medical Association, 1893; Professor of Gynecology in Barnes Medical College, St. Louis, Mo. Member of the Southern Surgical and Gynecological Association. 178 Central Avenue, Hot Springs, Ark.

1894.—JENNINGS, CHARLES GODWIN, M.D. Professor of the Theory and Practice of Medicine and Clinical Diseases of Children in the Detroit College of Medicine; Physician to St. Mary's Hospital, Department of Diseases of Children; Physician to St. Vincent's Orphan Asylum; Consulting Physician to the Woman's Hospital and Foundlings' Home; Consulting Physician to St. Luke's Hospital; Member of the American Pediatric Society. 457 Jefferson Avenue, Detroit, Mich.

1891.—JOHNSTON, GEORGE BEN, M.D. Professor of the Practice of Surgery and Clinical Surgery in the Medical College of Virginia; Surgeon to the Old Dominion Hospital; Physician to St. Sophia's Home for the Aged; Physician to St. Joseph's Female Orphan Asylum; Consulting Surgeon to the Richmond Eye, Ear, and Throat Infirmary; Vice-President of the Southern Surgical and Gynecological Association, 1892, President, 1897; Ex-President of the Richmond Medical and Surgical Society; President of the Virginia State Medical Society, 1897. *Vice-President*, 1897. 407 East Grace Street, Richmond, Va.

1893.—LAIDLEY, LEONIDAS HAMLIN, M.D. Professor of Gynecology in the Beaumont Hospital Medical College; Surgeon-in-Chief to the Protestant Hospital. 3538 Washington Avenue, St. Louis, Mo.

1898.—LANGFIT, WILLIAM STERLING, M.D. Surgeon-in-Chief to St. John's Hospital. 608 Preble Avenue, Allegheny, Pa.

1890.—LONGYEAR, HOWARD WILLIAMS, M.D. Gynecologist to Harper Hospital; Physician to the Woman's Hospital; President of

the Detroit Gynecological Society, 1889; Chairman of the Section on Obstetrics and Gynecology of the Michigan State Medical Society, 1892. *Vice-President*, 1893. 698 Woodward Avenue, Detroit, Mich.

Founder.—†LOTHROP, THOMAS, Buffalo, N. Y., 1899.

1896.—LYONS, JOHN ALEXANDER, M.D. Instructor in Gynecology at the Post-Graduate Medical School; Gynecologist and Lecturer to Nurses at the Chicago Hospital. 4118 State Street, Chicago, Ill.

1891.—MACDONALD, WILLIS GOSS, M.D. Lecturer on Operative Surgery and Instructor in Abdominal Surgery in Albany Medical College; Surgeon to the Out-door Department of the Albany Hospital. 27 Eagle Street, Albany, N. Y.

1891.—*MCCANN, JAMES, M.D. Pittsburg, Pa., 1893.

1898.—MCCANN, THOMAS, M.D Professor of Surgery, etc., Western Pennsylvania Medical College; Visiting Surgeon, Western Pennsylvania Hospital. 3745 Centre Street, Pittsburg, Pa.

1894.—MCGUIRE, EDWARD, M.D. Professor of Gynecology in the University College of Medicine; Gynecologist to the Virginia Hospital; Member of the Southern Surgical and Gynecological Association. 216 East Franklin Street, Richmond, Va.

Founder.—MCMURTRY, LEWIS SAMUEL, A.M., M.D. Professor of Gynecology in the Hospital College of Medicine; Gynecologist to Sts. Mary and Elizabeth Hospital; Fellow of the Edinburgh Obstetrical Society; Fellow of the British Gynecological Society; Corresponding Member of the Obstetrical Society of Philadelphia and of the Gynecological Society of Boston; Member (President, 1891) of the Southern Surgical and Gynecological Association. *Executive Council*, 1891–1892, 1895–1900; *President*, 1893. 1912 Sixth Street, Louisville, Ky.

Founder.—MANTON, WALTER PORTER, M.D. Professor of Clinical Gynecology and Adjunct Professor of Obstetrics, Detroit College of Medicine; Gynecologist to Harper Hospital and the Eastern Michigan Asylum for the Insane; Vice-President of Medical Board of the Woman's Hospital and Foundlings' Home; Consulting Gynecologist to the Northern Michigan Asylum, and St. Joseph's Retreat; Gynecic Surgeon to the House of the Good Shepherd; President of the Detroit Academy of Medicine, 1892–1894; President of the Detroit Gynecological Society, 1890; Fellow of the British Gynecological Society; Fellow of the Royal Microscopical Society, and of the Zoölogical Society of London. *Vice-President*, 1894. 32 Adams Avenue, W. Detroit, Mich.

Founder.—MAXWELL, THOMAS JEFFERSON, M.D. Professor of the Principles and Practice of Surgery and Surgical Clinics in the Keokuk

Medical College; Surgeon to St. Joseph's Hospital. 727 North Ninth Street, Keokuk, Iowa.

1893.—*Michael, Jacob Edwin, A.M., M.D. Baltimore, Maryland, 1895.

Founder.—Miller, Aaron Benjamin, M.D. Professor of Gynecology in the Medical Department of Syracuse University; Gynecologist to St. Joseph's Hospital, House of the Good Shepherd and Dispensary. *Vice-President*, 1899. 326 Montgomery Street, Syracuse, N. Y.

1896.—*Mooney, Fletcher D., M.D. St. Louis, Mo., 1897.

1890.—Morris, Robert Tuttle, A.M., M.D. Instructor in Surgery in the New York Post-Graduate Medical School and Hospital. *Vice-President*, 1892. 58 West Fifty-sixth Street, New York, N. Y.

Founder.—Moses, Gratz Ashe, M.D. (See Honorary Fellows.)

1894.—Murphy, John Benjamin, A.M., M.D. Professor of Surgery in the College of Physicians and Surgeons and in the Post-Graduate Medical College; Attending Surgeon to the Cook County Hospital and to Alexander Hospital. Residence, 3152 Michigan Avenue; Office, 400 Reliance Building, 100 State Street, Chicago, Ill.

Founder.—Myers, William Herschel, M.D. Professor of Clinical and Abdominal Surgery, Fort Wayne College of Medicine; Surgeon to St. Joseph's Hospital; Member of the American and the British Medical Associations; Member of the Pathological Society of London; Member of the International Congress of Gynecologists and Obstetricians; Member of the Chicago Medical Society. *Vice-President*, 1890. 157 West Wayne Street, Fort Wayne, Indiana.

1897.—Nichols, William R., M.D. Markham, Ont., Canada.

1886.—Noble, George Henry, M.D. Gynecologist to the Grady Hospital; Secretary of the Section of Obstetrics and Gynecology, American Medical Association, 1897; Member of the Southern Surgical and Gynecological Association. 186 South Pryor Street, Atlanta, Georgia.

1889.—Paine, John Fannin Young, M.D. Professor of Obstetrics and Gynecology in the School of Medicine, University of Texas; Obstetrician and Gynecologist to the John Sealy Hospital; President of the Texas State Medical Association, 1888; Vice-President of the Section of Public and International Hygiene in the Ninth International Medical Congress; Member of the American Medical Association and of the Southern Surgical and Gynecological Association. S. E. corner Broadway and Twenty-sixth Street, Galveston, Texas.

1899.—PANTZER, HUGO O., M.D. Gynecologist to the City Hospital and City Dispensary; Consulting Gynecologist to the Deaconess's Hospital, and to the Indiana State Hospital. 316 East Michigan Street, Indianapolis, Ind.

1880.—PEARSON, WILLIAM LIBBEY, M.D. 713 Union Street, Schenectady, N. Y.

1891.—PECK, GEORGE SHERMAN, M.D. Consulting Surgeon to the Youngstown City Hospital. *Vice-President*, 1896. 26 West Federal Street, Youngstown, Ohio.

1899.—PFAFF, ORANGE G., M.D. Adjunct Professor of Obstetrics and Diseases of Women in the Medical College of Indiana; Gynecologist to the City, Deaconess's, and St Vincent's Hospitals. 1337 North Pennsylvania Street, Indianapolis, Ind.

1898.—PORTER, MILES F., M.D. Ex-President Indiana State Medical Society. 47 West Wayne Street, Fort Wayne, Indiana.

Founder.—POTTER, WILLIAM WARREN, M.D., Consulting Gynecologist to the Woman's Hospital; Consulting Surgeon to the Buffalo General Hospital; President and Examiner in Obstetrics, New York State Medical Examining and Licensing Board; Chairman of Section of Obstetrics and Diseases of Women, American Medical Association, 1890; President of the Buffalo Obstetrical Society, 1884–1886; Member of the Southern Surgical and Gynecological Association; President of the Medical Society of the State of New York, 1891; Executive President of the Section of Gynecology and Abdominal Surgery, Pan-American Medical Congress, 1893. *Secretary*, 1888–1900. 284 Franklin Street, Buffalo, N. Y.

1891.—*PRAEGER, E. ARNOLD, M.D. Los Angeles, Cal., 1898.

Founder.—PRICE, JOSEPH, M.D. Physician-in-Charge of the Obstetrical and Gynecological Department of the Philadelphia Dispensary; Member of the Southern Surgical and Gynecological Association; Honorary Fellow of the Medical Society of the State of New York; Honorary Fellow of the South Carolina Medical Society; Honorary Fellow of the Virginia Medical Society; Member of the British Gynecological Association and of the Edinburgh Obstetrical Society. *Executive Council*, 1894–1895; *President*, 1896. 241 North Eighteenth Street, Philadelphia, Pa.

Founder.—REED, CHARLES ALFRED LEE, A.M., M.D. Professor of Gynecology and Abdominal Surgery in the Cincinnati College of Medicine and Surgery and in the Woman's Medical College of Cincinnati; Surgeon to the Cincinnati Free Surgical Hospital for Women; Secretary-General of the First Pan-American Medical Congress, 1893;

Member of the Southern Surgical and Gynecological Association. *Executive Council*, 1890–1897; *President*, 1898. St. Leger Place, Cincinnati, Ohio.

1896.—RHETT, ROBERT BARNWELL, JR., M.D. Dean and Professor of Gynecology and Abdominal Surgery at the Charleston Medical School; Gynecologist to St. Francis Xavier's Infirmary; Surgeon to the City Hospital. 109 Cannon Street, Charleston, S. C.

1890.—RICKETTS, EDWIN, M.D. Professor of Abdominal Surgery and Gynecology at the Cincinnati Polyclinic; Member of the American and British Medical Associations; Member of the Southern Surgical and Gynecological Association. *Vice-President*, 1899. 415 Broadway, Cincinnati, Ohio.

1889.—*ROHÉ, GEORGE HENRY, M.D. Baltimore, Md., 1899.

1892.—ROSENWASSER, MARCUS, M.D. Dean and Professor of Diseases of Women and Abdominal Surgery in the University of Wooster; Gynecologist to the Cleveland Hospital for Women and Children; Consulting Gynecologist to the City Hospital; Member of the American Medical and Ohio State Medical Association. 722 Woodland Avenue, Cleveland, Ohio.

1890.—ROSS, JAMES FREDERICK WILLIAM, M.D., L.R.C.P. (Eng.). Gynecologist to the Toronto General Hospital; Surgeon to the Woman's Hospital; Lecturer in Clinical Gynecology at the University of Toronto. *Executive Council*, 1892–1896; *President*, 1897. 481 Sherburne Street, Toronto, Ont., Canada.

1895.—SELLMAN, WILLIAM ALFRED BELT, M.D. Professor of the Diseases of Women and Children at the Baltimore University School of Medicine; Member of the Medical and Chirurgical Faculty of Maryland; also the Baltimore Medical and Surgical Association; the Gynecological and Obstetrical Association of Baltimore; the Clinical Society; the Baltimore Journal Club; the American Medical Association, etc. 5 East Biddle Street, Baltimore, Md.

1890.—SEXTON, JOHN CHASE, A.M., M.D. *Executive Council*, 1894; *Vice-President*, 1897. Rushville, Indiana.

1889.—SEYMOUR, WILLIAM WOTKYNS, A.B., M.D. Professor of Gynecology in the University of Vermont; formerly House Surgeon of the Boston City Hospital; Member of the American Medical Association; Fellow of the New York State Medical Association; Member of the British Medical Association. *Executive Council*, 1892–1893. 105 Third Street, Troy, N. Y.

1899.—SIMPSON, FRANK FARROW, A.B., M.D. Assistant Gynecologist to Mercy Hospital. 524 Penn Avenue, Pittsburg, Pa.

1891.—SMITH, CHARLES NORTH, M.D. Professor of Obstetrics and Clinical Gynecology in the Toledo Medical College; Gynecologist to St. Vincent's Hospital. 234 Michigan Street, Toledo, Ohio.

1895.—STEELE, DANIEL ATKINSON KING, M.D. President and Professor of the Principles and Practice of Surgery at the College of Physicians and Surgeons; Attending Surgeon at the Chicago, Wesley, and Post-Graduate Hospitals; Consulting Surgeon at the Palmer Memorial Hospital, Janesville, Wis. 2920 Indiana Avenue, Chicago, Illinois.

Founder.—†STORRS, MELANCTHON, A.M., M.D. Hartford, Conn. (See Honorary List, 1899.)

1894.—STOVER, CHARLES, M.D. 31 Division Street, Amsterdam, N. Y.

1899.—SWOPE, LORENZO W., M.D. Surgeon to the Consolidated Traction Company; Assistant Surgeon to the West Pennsylvania Hospital. 3609 Forbes Street, Pittsburg, Pa.

1894.—†TAPPEY, ERNEST TAYLOR, A.M., M.D. Detroit, Mich., 1899.

1894.—TAYLOR, HUGH MCGUIRE, M.D. Professor of the Practice of Surgery in the University College of Medicine, Richmond, Va.; Member of the Surgical Staff of the Virginia Hospital, Richmond, Va.; Ex-President of the Medical Examining Board of Virginia; Ex-President of the Richmond Medical and Surgical Society; Member of the American Medical Association; Member of the Southern Surgical and Gynecological Association; Member of the National Association of Railway Surgeons. 6 North Fifth Street, Richmond, Va.

1890.—THOMAS, GEORGE GILLETT, M.D. Ex-President Medical Society of the State of North Carolina. Wilmington, N. C.

1898.—THOMAS, JOSEPH DIO, M.D. Surgeon to the South Side Hospital. 77–79 South Thirteenth Street, Pittsburg, Pa.

1895.—THOMPSON, FRANK DANIEL, M.D. Professor of Gynecology in the Medical Department of Fort Worth University. 412 Adams Street, Fort Worth, Texas.

1895.—TOMPKINS, CHRISTOPHER, M.D., Ph.D. Professor of Obstetrics and Dean of the Medical College of Virginia; Obstetrician to the Old Dominion Hospital; Member of the Southern Surgical and Gynecological Association. 116 East Franklin Street, Richmond, Va.

Founder.—*TOWNSEND, FRANKLIN, A.M., M.D. Albany, N. Y., 1895.

Founder.—VANDER VEER, ALBERT, A.M., M.D., Ph.D. Professor of Didactic, Clinical, and Abdominal Surgery in the Albany Medical College; Attending Surgeon to the Albany Hospital; Consulting Surgeon to St. Peter's Hospital; Fellow of the American Surgical Association; Fellow of the British Gynecological Society; Member of the Southern Surgical and Gynecological Association; Corresponding Member of the Boston Gynecological Society. *Executive Council*, 1889–1891, 1895–1900; *President*, 1892. 28 Eagle Street, Albany, N. Y.

1891.—WALKER, EDWIN, M.D., Ph.D. Gynecologist to the Evansville City Hospital; President of the Indiana State Medical Society, 1892; Member of the American Medical Association and of the Mississippi Valley Medical Association; Member of the Southern Surgical and Gynecological Association. 427 Upper Third Street, Evansville, Indiana.

1889.—WENNING, WILLIAM HENRY, A.M., M.D. Professor of Obstetrics in the Woman's Medical College; Gynecologist to St. Mary's Hospital. 722 Laurel Street, Cincinnati, Ohio.

Founder.—WERDER, XAVIER OSWALD, M.D. Professor of Gynecology at the Western Pennsylvania Medical College (Medical Department, University of Western Pennsylvania); Consulting Gynecologist at the Allegheny General Hospital; Gynecologist to the Mercy Hospital and Pittsburg Free Dispensary; Obstetrician to the Roselia Maternity Hospital; Consulting Gynecologist to St. Francis's Hospital; Consulting Surgeon to the South Side Hospital. *Treasurer*, 1888–1900. 524 Penn Avenue, Pittsburg, Pa.

1896.—WESTMORELAND, WILLIS FOREMAN. Professor of Surgery at the Atlanta Medical College. Equitable Building, Atlanta, Georgia.

1895.—WHEATON, CHARLES AUGUSTUS, M.D. Professor of Clinical Surgery in the University of Minnesota. 301 Summit Avenue, St. Paul, Minn.

1897.—WHITBECK, JOHN W., M.D. Gynecologist to the Rochester City Hospital; Commissioner of the Board of Health. 322 East Avenue, Rochester, N. Y.

1897.—WILLIAMS, HENRY T., M.D. Attending Surgeon, City Hospital; Attending Surgeon, St. Mary's Hospital; Attending Surgeon, Monroe County Penitentiary; Consulting Surgeon to the Home for the Friendless. 52 Clinton Place, Rochester, N. Y.

Founder.—WRIGHT, ADAM HENRY, B.A., M.D. 30 Gerrard Street, East, Toronto, Ont., Canada. Transferred to Corresponding List.

Total, one hundred and one Ordinary Fellows.

MINUTES OF THE PROCEEDINGS

AT THE

TWELFTH ANNUAL MEETING

OF THE

AMERICAN ASSOCIATION

OF

OBSTETRICIANS AND GYNECOLOGISTS,

HELD IN THE

CENTURY CLUB ROOM OF THE DENISON HOUSE,

Indianapolis, Indiana,

SEPTEMBER 19, 20, AND 21, 1899.

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PRESIDENT'S ADDRESS.

THE RIGHTS OF THE UNBORN—THE PREVENTION OF CONCEPTION.

By EDWARD J. ILL, M.D.,
NEWARK.

With feelings of deep gratitude and just pride I want to thank the Fellows of this Association for the honor of electing me their presiding officer. To hold an office once graced by such as have been honored in the past is a matter of no small moment in one's life and should make one a life-long debtor to the Association.

Among the many shortcomings that Nature was pleased to endow me with was that of oratorical talent. This shortcoming I feel so much the more as my address to you follows that brilliant effort of our esteemed and learned President of last year, whose oratory has always excited my wonder and admiration.

In looking about for a subject that might interest you sufficiently, all thoroughly conversant with the thought and knowledge of our beloved art, I have been sadly reminded that every conceivable subject has been thoroughly raked over, and there seemed little left for me to say. Still, I hope that our conceitedness will not lead us to believe that we have reached that condition which the great French surgeon of nearly four hundred years ago thought he had arrived at when he said: "Nor has posterity anything left but a certain small hope to add some things."

There are a few things, however, which one might touch upon and which should more frequently and more thoroughly be discussed, if only to condemn the practice from the stand-point of the physician. Such condemnation would have the greater weight, even if it only found the silent acquiescence of such an association as ours. You will pardon me, therefore, for drawing your atten-

tion to a matter as to which in your own mind you have long ago come to the same conclusions as myself.

An abuse, fostered by the security that aseptic surgery gives us, has gradually crept into the work of many all over the civilized world. The rights of the unborn are often not respected as they should be and a strict regard for truth, conscientiousness, and careful professional thought on our part would indicate. The rights of the unborn would be more respected if it could secure for itself competent legal authority to represent it before a court of law. Again and again it has been taught that "whenever the mother is suffering from such grave disease that her life is in imminent peril and can be saved only by the arrest of gestation is induced abortion justifiable."

We are also told that the induction of abortion should never be done except with consent of consultant. But this law and custom leaves a wide field for opinion, judgment, and possibly "professional courtesy." Religious doctrines have no weight with me when the destruction of fetal life becomes a necessity to save that of the mother, since both would die if Nature was allowed to continue; but the line must be drawn so close that the mother is actually nearing the grave, and every means known exhausted to remedy the evil, before the decisive step is taken. Neither can convenience, professional courtesy, actual suffering on the part of the mother, nor social disgrace ever be an indication for what to me would be a most horrible crime—the murder of the defenceless. I have no doubt that what has occurred to me has happened to all of you who are working in this chosen part of the practice of medicine. We are called upon to induce an abortion for hyperemesis long before the mother has reached that stage when her life becomes endangered, simply because the family have forced the attendant to the wall with their sympathetic wails, or because the attendant himself has reached the end of his resources. His patience and his sympathy have run away with his reason, or, if we shall express ourselves less charitably, he has become tired of the case, but feels he cannot give it up to another. I can sympathize with him, for I know of no more aggravating condition, where for weeks and months our efforts are little more than comforting; but I cannot consent to be a party in the destruction of the unborn life so long as the mother is not in absolute danger,

and until I have personally used all resources at my command, which includes treatment away from home and systematic trained nursing. Not that I think my professional friends cannot do equally well, but my conscience must be quieted and I personally must have failed in the endeavor. With a large experience and much patience, I have thus far never been obliged to empty the uterus previous to the viability of the fetus, and I have never seen a mother die. Many cases have given me sleepless nights, as I often felt that the mother was nearing that brink where the uterine contents must be interfered with. I am sure that many more children should be saved. The frequent deaths of the unborn, as caused by the regular profession legitimately, must ever remain a sign of weakness and impotence of an otherwise noble and humane profession.

By artificial or induced abortion we should understand an interruption of gestation before that time when the child is viable—in other words, before the twenty-eighth week. Rare instances have been cited where children have remained alive before that time, but such reports should be received with much reserve.

The indication for such an operation must greatly change with the advances of therapeutics, be they medicinal or surgical. I am sure that before long we shall be told that the life of the fetus must not be taxed less than that of the mother. When that becomes an axiom our resources will increase to an astonishing extent.

Such changes have been very apparent during the past few years, and, to the honor of a lofty profession let it be said, not to the disadvantage of the fetus and without additional risk to the mother.

During the early professional life of nearly all of us it was deemed wise and just to destroy the fetus when the pelvic conjugate reached below a certain measure. Today we command that Cesarean section be performed, with the greatest likelihood of saving both mother and child, and with much less risk to the mother than was formerly incurred by induced abortion. Even repeated Cesarean section should be performed and is performed by those of advanced surgical technique and skill.

The same must be said of tumors blocking the pelvic outlet, carcinoma excepted under certain circumstances. In cases of large,

rapidly growing myomata and pregnancy, when the suffering of the mother becomes intolerable and her life despaired of, we should first attempt removal of the tumor by myomectomy, and, if that prove impossible, we may then resort to that which becomes inevitable. Thus the reader has been able to shell out a sixteen-pound tumor from a six-months' pregnant uterus without interruption of pregnancy.

Not infrequently one will be astonished to see myomata rise above the pelvic inlet in the last weeks of pregnancy, followed by perfectly normal labor.

We may be placed in an embarrassing situation when we consider carcinoma of the cervix. A little reflection, however, must lead us on to the right path. If the disease is discovered early during pregnancy, while a possibility of total extirpation exists, our course is clear. The mother's life should be saved, as the likelihood of a viable child is extremely doubtful.

If pregnancy is well advanced and all chances of radical operation for the mother frustrated and lost, it will be our duty to make an effort to save the child by Cesarean section, with little likelihood of shortening the life of the mother. Here we may feel obliged to wait for the child's viability. The rapidity with which carcinoma spreads after ordinary labor, and the likelihood of septic infection, make the probability of lengthening the life of the mother greater by suprapubic delivery. Statistically we know that out of 603 cases of carcinoma, 43.3 per cent. died intrapartum or post-partum.[1]

Arguments in favor of induced abortion not easily refuted are found in the non-replaceable, complete prolapse of the pregnant uterus. In the retroflexed incarcerated uterus, when even a celiotomy will not help us, we have no alternative as yet. Since it has been found that a few cases of artificial fixation have caused insuperable obstacles to normal delivery of the child, this might be cited as a possible indication for the induction of abortion; but Cesarean section is so easy and safe an operation that such an indication must fall at first sight. I am sure we will very rarely be called upon to induce abortion for chronic disease, and especially for nervous disorders. In the former there must be a wide field

[1] Veit: Handbuch der Gynäkologie.

for careful observation and rational therapeutics as well as good judgment.

When we find ourselves confronted with cases that need artificial termination of a pregnancy, let the indication be drawn so close that the most sensitive conscience will not find fault.

In regard to the mental condition of the laity concerning the induction of abortion, too much surprise cannot be expressed at their ignorance and laxity of conscience.

Persons of good repute, whose standing as good citizens in the community is not questioned, can see no harm, mentally, physically, or morally, in it. It remains for us to teach them that life begins with conception, for it is right here that ignorance of the subject is so stupendously apparent.

In what has just been said you are no doubt aware that the reader has criticised the profession in general for too great laxity in dealing with fetal life, and he hopes he has done so in such terms as will leave no misunderstanding.

There is another question we have to deal with. It is one that is not touched upon with pleasure, and therefore rarely mentioned as a factor in uterine diseases.

Marriage, as we understand it, is presumed to be the normal institution for the propagation of our species and the care and education of the progeny. If such is the case, I am sure we all have often been perplexed by the shameless confession of a handsome, and what is apparently a correct, young married woman that she prevents conception ; even more, that she entered the marriage bed with the distinct understanding that she desires no offspring, and does so because of the inconvenience it would give her. It has been my sad experience to note this antipathy to be more frequent in the young woman than in the young man. The depth of moral degeneracy in such cases can only be imagined. I have no patience with these women, and often direct that they may return for my advice and treatment when they have decided to live a natural life and have ceased to be legitimate prostitutes.

At times the physician himself advises prevention of conception for the most trivial or imaginary ailment. Social and economic reasons do not concern us in this matter.

When such hideous miscarriages occur in that God-given passion upon which all that is good, true, and noble in our lives

depend, there is something radically wrong in the education of the young. It is hard for us to disconnect the moral from the physical well-being. Though we have no right to chastise our patients for their morality, it behooves us to tell them where physical misery depends upon moral decrepitude and to decline our assistance in their illnesses unless proper regard be given to our admonitions. At the same time it seems difficult not to express our disgust for such abnormalities, especially when they have become daily practices. We understand that originally this unnatural and physically harmful procedure originated with those individuals who lay aside all sense of duty and live lives of pure egotism. With them neither Church, conscience, nor Nature has any power. They do not understand the sexual instinct in its higher and highest import. Most frequently they are devoid of heart and sympathy. If they could but understand that it is this instinct which causes the blooming of the lily in the valley and the roses by the wayside ; that it is this that produces the song of the nightingale and the chirp of the cricket ; that it is this that produces all the beautiful colors and forms of nature, and all that is beautiful in the form and intellectual power of mankind. All our poetry and beautiful literature, our social life and culture, art and science, and even our religion, finds its fountain sources and life in it. All progress in the races depends upon this God-given instinct, and certainly without it this life must remain a dreary desert.

You will pardon the digression into which I have been led, for it has not been my aim to speak of the moral condition of these patients, though it is hard to separate it from the physical. I want to draw your attention, however, to the pathological conditions produced by artificial sterility. Looking over my note-books, I am reminded that fewer women suffer who already have had several children than those who begin life with this nefarious practice. Without endeavoring to explain the *rationale* of the pathological condition, we can trace a chain of subjective symptoms to a local condition by combining a number of cases from one's case-book. It becomes very apparent that the practice is the cause of the train of symptoms to be enumerated when we are aware that women living normal sexual relations, but sterile, do not suffer in such typical manner.

We find ourselves confronted with two sets of symptoms—nervous and local. The latter are those which precede the former. Among the local symptoms we find increased mucous discharges from the genitals, frequent micturition, bearing-down pubic pain, pain in both iliacs and the small of the back. Most commonly this is the order in which the symptoms come on. Now and then the backache will precede or accompany the leucorrheal discharge.

Standing and walking become painful ; thus, owing to lack of exercise, there results a condition of slowness in the action of the bowel, a disturbed digestive and heart action. With failure of the general health the nervous system fails. These patients tell us of several attacks of nervous prostration. A physical exploration shows increased vaginal discharges ; the vagina appears red and sensitive, its mucous lining thickened. The cervix and corpus are enlarged. The cervix looks blue, with turgescence of its veins, and an abundant quantity of blood is poured from them as soon as they are punctured. The quantity of glairy mucus which pours from the cervix is greatly increased over normal.

Frequently the uterus is retroflexed, rarely retroverted, and the utero-sacral ligaments so sensitive that they can barely be touched. With these symptoms we nearly always find enlarged, prolapsed, and sensitive ovaries. Among the early symptoms will be an increased menstrual flow, and with it often an easing-up of the congestive symptoms. Later this becomes less than normal and makes the prognosis an exceedingly bad one. One can safely say that when this stage is reached the patient has become an invalid and permanently sterile. In aggravated cases menstrual pain begins several days before the flow, and is marked by radiations from the iliac fossæ down the anterior and inner aspect of the thighs. Now we find them complaining of a peculiar pain midway between two periods. In these bad cases intercourse has become painful and often disgusting, so that a certain amount of coldness between the couple has become apparent.

Neugebauer asserted at the International Congress of Medical Sciences, held at Amsterdam in September, 1879, that the origin of cancerous growths can be traced to the practice in question. I have never been able to verify the statement, but one can easily imagine the possibility.

It has been my experience, however, to see a number of cases of

myomata in women of thirty years and less who confessed to have followed such practices.

I have had a curious experience with some whom I thought had reached the sterile period, inasmuch as they became exceedingly desirous of offspring. This being denied them, they became fruitful patients to a succession of gynecologists.

Nervous symptoms manifest themselves variously, from occipital and vertex headaches and minor hysterical manifestations to the graver forms of hysteria and semi-melancholia.

It is true that many continue to live in this fashion with apparently no physical harm. One can commonly notice, however, a dissatisfaction with the world and its manifold interests even in this class. The intimate bond of union between man and wife is missing.

THREE RARE CASES OF KIDNEY CYSTS.

By J. F. BALDWIN, M.D.,
COLUMBUS.

CASE I. *Enormous hydronephrosis simulating ovarian cystoma.* —Mrs. M. A. H., Columbus, Ohio, referred to me by Dr. S. O. Giffin, October 17, 1898, aged fifty-six years ; married for twenty years ; a widow for the last fourteen years ; mother of two children, the youngest twenty-four years of age ; labors normal ; no abortions ; general appearance good ; bowels regular ; appetite poor ; has had some irritation of the bladder at times ; passed the menopause normally at forty-eight. For the last two years had noticed that she was getting somewhat larger, but supposed that it was ordinary stoutness incident to her age. A month ago had a fall down the cellar stairs. Since that time has had some pain in the right side of the abdomen and has noticed a somewhat rapid increase in size. This led her to consult her physician, who found a tumor present. This tumor is so large that the patient presents the appearance in general of a woman pregnant eight months. The tumor is not entirely symmetrical in character, being decidedly more prominent on the left side of the median line. It extends down into the pelvis, pushing the uterus to the left. The patient does not know of any starting-point of this tumor, but only knew of its existence when her physician found it on her visit to him yesterday. Fluctuation is distinct throughout the entire mass, and the diagnosis of an ovarian cystoma seems perfectly plain.

The patient was operated upon at her residence October 19, Dr. Giffin administering the anesthetic and Drs. Chapman and Lyne assisting. The usual abdominal incision was made a little to the right of the median line, just large enough to admit a couple of fingers, with the expectation of tapping the cyst and drawing it out through the small opening. The introduction of the fingers showed that the tumor, while dipping down into the pelvis, had

no pelvic connections and was entirely retroperitoneal. The incision was therefore enlarged until the hand could be introduced, when it was found that the mass, though apparently much larger on the left side of the abdomen, originated on the right side. The tumor was tapped with a large trocar and about ten pints of fluid drawn off. The fluid evidently contained some recently effused blood. The sac being now collapsed, the opening in the visceral peritoneum was enlarged, the hand introduced, and the sac enucleated. It was found attached throughout to the connective tissue of the ascending mesocolon, the head of the colon and the ascending colon being, indeed, under the line of the original incision. The tumor extended up under the liver, and was enucleated with considerable difficulty from this region. When enucleation had progressed about one-third of the way down a very small blood-vessel was encountered, which, when torn across, spurted. It was very small and was ligated with fine catgut. This was the only vessel needing any ligature. As the enucleation progressed a pedicle was finally reached extending down to the bottom of the pelvis. This pedicle, when brought up, was found to resemble very closely a piece of small intestine. Tracing this down it was found to enter the pelvis behind the head of the colon and the last portion of the ileum. More careful study showed that the cyst itself was simply an enormous hydronephrosis, and that the pedicle, resembling a piece of small intestine, was the dilated ureter. The tumor was therefore removed, the pedicle being caught with forceps; a buttonhole was torn through the peritoneum deep down in the pelvis, and the ureter drawn through this opening and separated to the base of the bladder. Just at this moment the patient made a sudden movement, so that it was torn off just above its point of entering the bladder. An opening was made through Douglas's cul-de-sac, this remaining bit of ureter caught with forceps and drawn into the vagina with a wisp of gauze to hold it in place. The space left after the removal of the cyst had collapsed, and there was no hemorrhage whatever. The peritoneal opening through which the hand had passed fell naturally together and was left unsutured, so that if any hemorrhage occurred it would find an exit into the general peritoneal cavity. Before closing the abdomen the region of the right kidney was explored and no vestige of any organ found. The left kidney was *in situ*. The abdomen

was closed without drainage. The patient made an uninterrupted and rapid recovery. More careful examination of the sac removed showed that it contained no vestige whatever of kidney structure or of large bloodvessels. It is evident that we had here a very old hydronephrosis; that as a result of the fall there was a hemorrhage into the sac, accounting for the pain which she felt and the rapid increase in size. Subsequent close interrogation failed to elicit any history of kidney trouble.

A more careful study of the case in the light of post-operative knowledge has not enabled me to see how any other diagnosis than the original one of ovarian cystoma could have been arrived at.

CASE II. *Large paranephric cyst.*—Mrs. S. C., referred to me by Dr. Kirkpatrick, of London, O., December 13, 1898; aged thirty-three years; married five years; never pregnant; general appearance fair; appetite poor; bowels constipated; some irritation of bladder at intervals; menstruation fairly normal. Was sick with something resembling a local peritonitis of the right side last June. Has never been quite free from some local soreness in this region since that time. Was examined by her physician this morning, who found an enlargement on the right side and sent her to me. Examination at this time shows a cystic tumor in the region of the right kidney, about the size of a fetal head. It is somewhat tender on pressure, especially at its lower pole. It is in this region, she says, that she has had the tenderness during the last six months. A diagnosis of hydronephrosis seems natural, although from the family history of the patient tuberculosis cannot be overlooked. Operation was advised, and was made December 15th at the Protestant Hospital, Dr. Chapman assisting and Dr. Kirkpatrick being present. As a preliminary Harris's instrument for securing the urine from each kidney was introduced. During the few minutes of its remaining *in situ*, and while about a drachm of urine came from the left kidney, only a drop or two appeared from the right. This seemed to confirm the idea of a diseased condition of the right organ.

An incision was made in the loin as usual. As the knife was being used to enlarge the opening slightly downward into the deeper parts, a cyst was incised, which gave exit to a fluid resembling urine in every way. The cyst wall was seized with forceps and the entire cyst enucleated, though not without some difficulty.

It had no pedicle and no special vascular supply. The cyst being removed, it was found that the previously existing mass had entirely disappeared. By bimanual examination, with the fingers of the left hand introduced into the incision, the contour of the right kidney could be easily made out. This kidney was seemingly normal in size and structure. The cyst removed had an exceedingly thin wall, scarcely thicker than the lining of an eggshell. The contents, which had escaped, were estimated to have amounted to about a pint and a half. The incision was closed except a space for a wisp of gauze, which was allowed to remain for a few hours, its removal being followed by union of the wound by first intention. Examination of the patient some months later satisfied me that the inflammatory trouble which she had had, and the attacks which followed, were all the result of appendicitis, from the chronic form of which she still suffers, and for the relief of which an operation has been advised and will probably be made.

The cyst was clearly, I think, what is known as a paranephric cyst. I have been able to find very little literature on this subject, but Henry Morris gives a fairly satisfactory description in his little work on the *Surgical Diseases of the Kidney*. He says they are neither developed in the kidney nor are they due to a dilatation of the renal pelvis. Although they sometimes have a connection with the interior of the kidney, this connection is supposed to be secondary in the development and to be really a fistulous passage. He reports one case which was found at autopsy during the records of ten years at the Middlesex Hospital. Patient died of carcinoma of the small intestine, obstructing the ureter and producing dilatation of the pelvis of the kidney. Behind this kidney there was a large cavity containing clear fluid; the anterior wall of this cavity being formed by the kidney capsule, the posterior and lateral walls consisting of condensed paranephric cellular tissue. A small passage extended from the cyst to the dilated renal pelvis. While little seems to be known about these cysts, it is not difficult to regard them as due to the presence, outside of the kidney and unconnected with the ureter, of a minute mass of congenitally misplaced kidney tissue. The slight secretion of urine would result in the formation of the cyst, which, becoming greatly distended, would cause obliteration of the kidney tissue, or at least its change beyond macroscopic recognition.

CASE III. *Cyst of a retrorectal congenitally misplaced and sarcomatous kidney.*—Mrs. F. D. M., referred to me January 2, 1899, by her physician, Dr. A. H. Kreager, of Nashport, O. Patient aged sixty years; mother of five children, the youngest eighteen years of age; labors normal; no abortions; general appearance fair; appetite poor; bowels opened with extreme difficulty, owing to the presence of a tumor; urine normal, but urination impossible except with catheter, owing to the tumor. First noticed that something was wrong in her pelvis about the last of October. At that time, while lifting a basket of peaches, felt something give way in her pelvis. She can tell very little about its rapidity of growth. Has lost a good deal of flesh, especially during the last two weeks. Her legs pain her some, but they are not swollen. On examination find a large, smooth mass filling Douglas's cul-de-sac, pushing the womb up out of reach and pressing firmly against the pubes, so much so as to obstruct the passage of urine, though the finger can be forced between the tumor and the symphysis. Rectal examination seems to show the rectum passing back of the tumor and flattened out by it. The tumor is immovable, but clearly cystic. Diagnosis is that of an ovarian cystoma incarcerated in the pelvis. Operation was made January 4, 1899, at the Protestant Hospital, Dr. Chapman assisting, Dr. Kreager being present. An attempt was made to open the cyst through the vagina. It was found, however, that the rectum, which at its lower end seemed to pass up behind the tumor, really made here a mere pouch, and above was spread out in front of the cyst. Working to one side with the fingers, the rectum was pulled over out of the way, but on trying to separate the adhesions somewhat more extensively, the finger entered the lumen of the bowel. The abdomen was then opened, when it was found that the tumor was a cyst filling the pelvis posteriorly and located behind the rectum and pelvic peritoneum. Uterus and ovaries were normal. The tissues over the tumor were exceedingly vascular, so much so that it seemed wise to again attempt its removal per vaginam. The cyst was accordingly tapped through the opening which had already been made, and over a quart of straw-colored fluid, resembling urine, was drawn off. The tumor being collapsed, its wall was seized and enucleation attempted, but the adhesions were too firm beyond the reach of the fingers. Entrance was therefore

again made through the abdomen, the peritoneum incised, and the upper portion of the tumor enucleated. The firmest adhesions were in the region of the sacrum, and all the adhesions were very vascular, so that the hemorrhage was quite profuse. The tumor, being entirely enucleated, was removed through the abdomen. Hemorrhage was controlled by packing with gauze sponges, and finally by a mass of gauze packed firmly into the cavity, the end being passed out through the opening into the vagina. The peritoneum was then carefully closed over this gauze with catgut, the pelvic cavity cleansed, and the abdomen closed in the usual way. The patient's recovery was uneventful, though somewhat tedious, owing to the drainage inserted in the postrectal cavity. The opening into the rectum was small and made no trouble. Under date of September 15th, Dr. Kreager writes me that the patient is still in excellent health, has increased somewhat in weight, and is able to do the housework for a family of seven. She has no trouble with her kidney, so far as she knows, but passes about one quart of urine per diem.

Examination of the specimen removed showed it to be a sarcomatous kidney with hydronephrosis as a complication. While the sarcoma seemed to have destroyed all the kidney structure, so that the microscopic examination was somewhat in doubt, examination of the patient before her return home, by means of the Harris instrument, showed no secretion whatever from the left kidney, while a normal amount was coming from the right.

Several years ago I saw a male patient, in consultation with Dr. Adams, of this city, who had a tumor filling the left side of the pelvis, closely resembling in its physical characteristics an ordinary multilocular ovarian cyst. The left testicle in this patient had never descended, and the diagnosis of sarcoma of an undescended testicle seemed to be very clear. The growth finally extended well up into the abdomen. The autopsy showed a normal testicle embedded in a mass of sarcoma which had originated in and entirely destroyed the left kidney. In this case we had evidently, I think, a pelvic kidney which had become sarcomatous, as in the case which I have just recorded.

A brief synopsis of this case was sent to Mr. Tait, of Birmingham, and to Drs. Kelly and Price, of our own country. All of them replied promptly to the letter of inquiry, stating that they

had never seen or heard of such a case. Mr. Tait replied at considerable length, inclosing a diagram showing how it might have occurred from a misplacement of a portion of the kidney congenitally malformed. He evidently misunderstood the report and thought that the pelvic kidney tissue was in addition to that in the normal position.

These cases are submitted because of their rarity. Quite a number of cases have been reported in which hydronephrosis has been mistaken for ovarian cyst. Paranephric cysts seem to be much more rare. The third case which I have reported is, so far as I can learn, entirely unique.

DISCUSSION.

Dr. James F. W. Ross, of Toronto.—Mr. President: I have been much interested in this paper, and I desire to mention one point in connection with the diagnosis of cystic tumors of the kidney, such as the Doctor referred to, and I do not know that there is any exception to the rule. It is this: Given a tumor that is unilocular by fluctuation, but apparently multilocular, on account of the striæ that often cross it, it is always a tumor of the kidney and not one of the ovary.

With reference to misplaced kidney, I have had one case of misplaced kidney suppurating in the pelvis.

One other point with reference to the treatment of the ureter. I think if the ureter is stripped down to a considerable distance, and its end tied with catgut, it can be left in a case of nephrectomy, and I do not think it is necessary to follow it down to the bladder unless markedly affected with tubercle.

The most important question in cases of paranephric cysts I have met with is that they are oftentimes congenital.

In these cases in which cysts are found, one should be certain that the quantity of urea excreted is not diminished to too great an extent before doing a nephrectomy.

I remember one such patient with a cyst of this kind, a young girl. Fortunately, I did not do a nephrectomy. Inside of several weeks a cyst showed itself in the other kidney, and I also opened that, but it contained pus. I drew out the cyst-wall of the first one operated on, cut off a portion of it, left a drainage-tube in for a short time, and she made a perfect recovery until, in a few weeks, the other kidney became the seat of an abscess. Had I removed this kidney at the first oper-

ation she would in all probability have lost her life, because there was not sufficient kidney tissue in the left kidney to sustain life.

Dr. Lewis S. McMurtry, of Louisville.—Dr. Baldwin's cases are very interesting, and I have had an experience which is exactly a repetition of his first case, the one of large renal cyst, which I gather from the report of the Doctor was considered to be an ovarian cyst when the operation was done. I do not believe there is any way to make a differential diagnosis of such cysts as the Doctor has reported, and I recall many cases as the analogue of his case.

I quite concur in the remarks of Dr. Ross relative to cysts of the kidney where we have the physical signs of monocysts, yet there will be found the folds across as he has described. These are significant and of very great aid. But in my own case the physical signs were distinctly those of a large ovarian monocyst. It was very much like the one reported by Dr. Baldwin, except that there was a very extensive vascular supply in the pedicle.

I agree with Dr. Ross in regard to the manner of dealing with the ureter. In a number of cases of nephrectomy, and in this case of large cyst, which have come under my observation the ureter was not dilated as much as Dr. Baldwin has described. I did not attempt to trace the ureter to the bladder and remove it all, but left it, and it did not give any trouble. In my own case the operation for the removal of the cyst was done six years ago, the fourth day of last July. The woman was twenty-four years of age; the cyst was very large, and the contents measured fully three quarts. She has since married, and is now in excellent health.

In regard to the case (the second or third one) in which the tumor was in Douglas's space, and for which the Doctor did the combined operation by the vagina and abdomen, his experience is a strong vindication of the superiority of the suprapubic route in dealing with all doubtful cases of intrapelvic tumors. In this instance by operating through the vagina the Doctor tore into the rectum, encountered extensive adhesions, then opened the abdomen, and, if I remember rightly, returned to the vagina again, finally removing the tumor through the abdominal route. Is that correct, Doctor?

Dr. Baldwin.—Yes.

Dr. McMurtry.—His experience is interesting and valuable to us all. It is much better to deal with any doubtful tumor occupying this space, even if it be exploratory, through the suprapubic route.

Dr. L. H. Dunning, of Indianapolis.—There is one important diagnostic point in dealing with cysts and tumors of the kidney that has not been mentioned, and that is the location of the colon in re-

spect to the cyst or the tumor. I once encountered a tumor that was diagnosticated by me as ovarian, and it turned out to be a hydronephrosis. Since then I have operated on five or six cases of tumors of the kidney, hydronephrosis, pus accumulations in the kidney, and sarcomatous kidneys, and in no instance of tumor of the kidney have I ever found the absence of resonance due to the presence of the colon over some portion of the tumor. In a few instances I have been able to demonstrate the presence of the colon in large sarcomatous tumors of the kidney in front of the tumor by pumping the colon full of air. In this way you can see the distended colon running over the outer border or centre of the tumor.

A few years ago we had a controversy as to the nature of a large tumor in a child two years of age. One said it was a sarcoma of one part, and another of another part. We decided that there was no question about its being a tumor of the kidney on account of seeing the colon upon the outer border of the tumor. The latter diagnosis we confirmed by operation. I regard this as a diagnostic point of great value, and I do not believe we should make an error in diagnosis very often if we remember this point.

With reference to dealing with the ureter, I have removed three kidneys for tuberculosis, in one of which I did not tie the ureter. I did not dissect it and remove it far down toward the bladder. I had considerable trouble for some time after. I think it is unsafe in tuberculosis of the kidney to leave the ureter, believing it ought to be removed low down in the pelvis and tied wherever it is left, and not followed clear down to the bladder. In other cases I have had no difficulty. I have extirpated the kidney eleven times with but one death.

DR. ROSS.—What ligature did you use in that case?

DR. DUNNING.—I used silk. Perhaps it was a mistake.

DR. J. HENRY CARSTENS, of Detroit.—We have all had more or less difficulty in making a diagnosis of cysts and tumors of the kidney, and the point brought out by Dr. Dunning is a good one. It does, not, however, always hold good. I recall one case in which I supposed I had to deal with an ovarian tumor, but it turned out to be an immense hydronephrotic kidney as large as Dr. Baldwin's first case, perhaps larger. The woman, after carefully investigating the history, undoubtedly had a floating kidney. At any rate, the kidney was evidently very loose, and afterward a stone developed in it, which was quite large. When we have a kidney that flops around it may grow over the colon; the colon in such a case is behind and below and not in front of the tumor. It does not always shift itself.

I recall another case of sarcoma of the kidney which I had in the northern part of this State (Indiana), which extended down to the pelvis. The history of enlargement of the spleen was quite clear, and the case was brought to me for the purpose of removing that organ. It seemed to have started a little on the left side; I examined the patient carefully, and had no doubt at all that it was really an enlarged spleen; still the blood tests showed that it was not the spleen which was at fault. I operated and found a malignant tumor (sarcoma) in front of the colon. It was post-peritoneal. I removed it without very much trouble.

I had one other case of large sarcoma on the right side a good many years ago and supposed it was a fibroid tumor. This tumor shoved everything aside, so that it was impossible to get a tympanitic sound anywhere. While the diagnostic point mentioned by Dr. Dunning is a good one, we must not look upon it as being reliable in all cases.

Dr. Joseph Eastman, of Indianapolis (by invitation).—I regret exceedingly that I did not reach the room in time to hear Dr. Baldwin's paper. However, the statement of Dr. Dunning brings to my mind some experience I had recently which may be of interest to the Association. The late Dr. Charles T. Parkes, of Chicago, in classifying tumors of the abdomen, spoke of those which grow from below upward, and those which grow from above downward. It has been my experience that all post-peritoneal tumors, such as spring from the kidney, are much less acted upon by the ascent and descent of the diaphragm than those located within the peritoneal cavity. I recently had a case of pancreatic cyst as large as my head or larger, and it was very satisfactory indeed to notice the ascent and descent of this tumor by the inspiration and expiration of the patient—a thing not possible if the tumor had been post-peritoneal or had been connected with the kidney.

Dr. Dunning suggests that the colon is in front of the tumor and can be dilated. This pancreatic cyst had distended the mesocolon to such an extent that the transverse colon was very perceptible, lying across in front of the large pancreatic cyst; so the diagnostic point which Dr. Dunning makes is applicable to pancreatic cysts. I am sure the ascent and descent of the diaphragm, although it is not marked in large growths of the kidney as of the pancreas, will be of decided advantage. This last case, which I operated on a week ago, impressed that on my mind forcibly; the ascent and descent of the diaphragm by inspection is much more pronounced than in any post-peritoneal growths connected with the kidney.

Dr. John M. Duff, of Pittsburg.—I recall a case of cyst of the

kidney at the McKeesport Hospital, which I had not seen previous to the day of operation, and was invited to assist in an operation for what had been diagnosticated as an abscess of the spleen. The urine had been examined and found normal. The tumor was almost in the median line, fluctuated, and it misled because the urine was reported as being normal. Nevertheless, I stated as my belief that it was a tumor connected with the kidney instead of the spleen. A median incision was made; the tumor was so large as to push up the mesocolon, and the colon itself was adherent to the tumor around at the side, the mesocolon having almost ulcerated through. The kidney was taken out through the mesocolon. There was no secretion of urine from the kidney, an examination not having been made with the Harris instrument for the purpose of ascertaining the condition of the urine from both kidneys. The patient made a good recovery.

Dr. Thomas, a Fellow of this Association, a short time ago had an interesting case, which I saw with him and assisted in operating upon, in a male in whom the tumor was very large. For a certain period it would become smaller, and then again enlarged. The history showed at the time that it was growing smaller; the patient passed a large amount of urine for a few hours. The doctor first aspirated, and found that he got urine from the tumor. He then made an incision and found a cyst which, I presume, held almost a gallon, which came off from the ureter about two inches below the kidney. I do not know whether the doctor has reported this case in any medical journal or not, but it was interesting to me. I have since seen the patient on the street, but I do not know just what his condition is.

Dr. Dunning.—I would like to ask Dr. Baldwin as to the value of the Harris instrument in examining the urine, and whether he thinks it is reliable or has been satisfactory in his hands.

Dr. Baldwin (closing the discussion).—I am much obliged to the Fellows for the freedom with which they have discussed my paper, but especially for the prominence given to the diagnosis in these cases, because this is one of the points of chief importance.

As to the diagnostic point mentioned by Dr. Ross, I have noticed several times in kidney tumors that we had a sort of a separation of the tumor into somewhat distinct masses, owing, I suppose, to the natural separation that exists between the lobules of the kidney. In my case, however, the cyst was so enormously distended as to fill the entire abdomen into the true pelvis. All partitions were entirely effaced, and the tumor was almost as symmetrical as an eight-months' uterus.

In one of the cases reported by me the cyst was retrorectal, and

had developed between the folds of the mesorectum, being in this respect, so far as I can learn, entirely unique.

In the first case which I reported I dissected out the ureter. It was distended to such an extent as to resemble a piece of small intestine precisely, and was filled with quite thick blood, owing to hemorrhage into the sac and the descent by gravity of the thicker portion of the blood into the ureter. It was, of course, obstructed at its point of entrance into the bladder. If I had closed and left it, I would have had an elongated tube without any opening at either end. This tube being filled with blood might easily become a source of infection, and it therefore seemed safer to remove it. It was dissected out without any difficulty and without materially prolonging the operation. It has not been my custom at all to dissect out the ureter when operating for the removal of tubercular kidney. I made such an operation once, but found it exceedingly difficult, and shall not likely repeat it. In these cases I have left the upper end of the ureter alone. There has been no opening of the peritoneal cavity, and the presence of the ureter has never seemed to interfere with the healing process. I use no ligatures of any kind in these cases, but apply a clamp to the pedicle of the kidney, including the ureter or not, as is most convenient. The clamp is left on for forty-eight hours, and I have never found its removal followed by any hemorrhage. By the use of the clamp nothing is left behind which might result in the formation of a sinus. If ligatures are used, in a tubercular case at least, they will certainly become infected, while catgut is no better than the clamp, and not nearly as convenient. I use the clamp the same as in a hysterectomy, and have had no hemorrhage and no delay in healing. I presume I have operated upon at least two dozen of these cases in this manner, and feel perfectly safe.

The paranephric cyst had no connection with either the kidney or the ureter, as in the case that Dr. Duff has marked out here on the black-board. I suppose that in my case a little portion of the kidney had been left in the pre-natal development of the woman, and had produced its secretion slowly, and by pressure there had been finally a destruction of all kidney tissue. I have no doubt the cyst had been present many years. It was easily dissected out considering the thinness of the sac. No one would think of making a nephrectomy in a case of that sort. I have seen a number of cases of cysts connected with the kidney, directly hydronephrotic in character. These I have incised, packed with temporary drainage, and the wound has healed kindly, leaving the kidney intact. I should hesitate to remove a kidney containing such cysts unless it seemed to be thoroughly de-

stroyed by the large number present. I have a patient under observation at the present time with two such kidneys, each nearly the size of a fetal head. The woman is over seventy years of age and in good health.

Dr. McMurtry spoke of the advisability of attacking these tumors always by the suprapubic route. In this case when I commenced the operation I supposed I had a cyst of the ovary in Douglas's cul-de-sac, pressing the uterus up and forward. It was low down on the pelvis, right at the vulva, apparently, and could have been easily removed through the vagina had my diagnosis been correct. When I made my incision, however, and dissected my way in, I found I had the rectum spread out in front of the tumor, although it had seemed to pass up behind it. The cyst was behind the rectum, and the adhesions were exceedingly vascular. As I had tapped the tumor from below, when I opened the abdomen I found it collapsed and was able to make an opening between the vessels and enucleate it.

Dr. Dunning spoke of the relation of the colon to these tumors. I had the honor of presenting a paper on that subject before the Ohio State Medical Society this year, in which I reported a series of cases of sarcoma of the kidney, and brought out that point in the diagnosis, namely, that the colonic tympany could be distinguished in front of or outside of one of these enlarged kidneys. Had I inflated the colon in my case I would have detected colonic tympany in front of the tumor, but the colon was so flattened out that no such tympany was present. The diagnosis, however, was so plain that it did not seem wise to undertake to dilate the colon for that purpose. I can recall a number of cases in which the diagnosis was established by the point which Dr. Dunning has mentioned of finding colonic tympany. I was recently called to operate on a case which had been diagnosticated one of splenic tumor. The physician, a very intelligent man and a surgeon of more than local reputation, asked me to operate. I asked him whether he was sure of his diagnosis, and he said he was. He had studied the case carefully, a blood examination had been made, and he was positive it was a case for splenectomy. I asked him if he could exclude the kidney, and he replied that he could. When I saw the case, however, I discovered colonic tympany on the outside of the tumor, and at once decided that it was a kidney instead of the spleen. The accuracy of this diagnosis was demonstrated during the later steps of the operation.

In reply to the question of Dr. Dunning as to the reliability of the Harris treatment, I will say that I have used it ever since it was brought before the profession. I do not remember in how many cases

I have used it in the female. I have used it a few times in the male, and have found it on the whole quite satisfactory. It is quickly used, its use is attended with less trouble than accompanies ureteral catheterization, and with less risk. In the case of the paranephric cyst I was deceived by it, for the reason that I did not leave it in long enough. The pressure of the cyst on the ureter as the woman lay down unquestionably obstructed the flow of urine from the ureter. The result was that while I was getting at least a drachm of urine from the left kidney I obtained only a drop or two from the right. I therefore concluded that the function of the right kidney had been practically lost, and yet when I made my operation I quickly saw how I had been deceived. Had I waited a few minutes longer before withdrawing the instrument, until there had occurred a sufficient accumulation above, I would have found the normal amount of urine secreted by the right kidney.

ECTOPIC GESTATION.

Shall the Case be Operated Upon at or Near Term, the Child Being Alive?

By L. H. DUNNING, M.D.,
INDIANAPOLIS.

As preliminary to a discussion of this subject, I wish to report the history of a case of ectopic gestation, of eight months' duration, and operated upon five weeks after the death of the fetus. The patient, Mrs. A., aged thirty-four years, had been married five years, and was a woman of good health until she became pregnant. She menstruated last on April 7, 1898, and signs of pregnancy appeared a little later. In the latter part of June she had uterine hemorrhages and irregular pains, indicating threatened abortion. A few days later she passed membranes, and within a few days became quite ill, and her physician thought there were retained membranes and placenta. He dilated the uterus and curetted it, removing some portions of the retained products of gestation, undoubtedly portions of decidua.

The following day the patient became very ill, was prostrated, pale, and had a rapid pulse. Fever followed. Combined examination showed a mass in the pelvis of considerable size. I saw her in consultation with her physician the latter part of June, and again about a week later. I halted in my diagnosis between suppurative disease of the tubes and ovaries and ruptured ectopic gestation. I was about to leave the city on my summer vacation, and asked to be released from the case. Dr. Pfaff was called in consultation the next day after my visit and made a diagnosis of ectopic pregnancy, probably interstitial. I lost sight of the case until November 26th, when I was called by Dr. A. L. Wilson to

visit the patient in consultation with him. Dr. Wilson had made a diagnosis of ectopic pregnancy with a living child.

I fully concurred in his diagnosis. The abdomen was as large as at an eight months' pregnancy, the outlines of the fetus could be felt and the fetal heart-sounds were distinct. On consultation we decided to await the death of the fetus and the cessation of placental circulation.

December 2d the patient had symptoms of labor, and after a few hours had a hard chill, which was followed by fever lasting several days. After the chill the patient felt no more movements of the child, and Dr. Wilson could not detect the fetal heart-sounds. I saw the patient December 14th. The abdomen did not seem to be as large as at the last visit. There had been a cessation of the fetal movements, and I was unable to hear the fetal heart beat. The patient was kept under close observation.

December 18th the right limb began to swell, and well-marked phlegmasia dolens developed. The patient suffered much pain and had considerable fever. A week later the left leg took on the same condition. Under appropriate treatment these lesions subsided. In the meantime the abdomen diminished in size and the fetal parts could be distinctly palpated. The head of the fetus lay well on the right side, and could be felt as a hard ball midway between the umbilicus and crest of the ilium, and the body could be traced around to the top of the mass, so we concluded the placenta was beneath the fetus.

She came to my hospital January 5, 1899, and was operated upon January 7, 1899. Upon opening the abdomen there was found a large, fleshy-looking tumor, occupying the lower two-thirds of the abdomen and slightly more prominent on the left side. The uterus was attached to the anterior surface of the tumor by bands of adhesion.

After carefully studying the case I concluded to enucleate the sac. The tumor (fetus and envelope) seemed to have a covering above, and there was a space between the uterus in front and intestines behind of four or five inches, over which was spread this covering of the sac. The tissue composing this covering was very vascular and looked like thickened musculoperitoneal tissue. I believed it to be a portion of the broad ligament or an expansion of the left Fallopian tube. It proved to be both. It was cut

through and a finger slipped under it between it and the sac proper, from which it was readily separated.

An incision about five inches long was now made through this covering, diagonally across the top and in front of the tumor from intestinal attachments behind to uterus in front, and the process of enucleation begun and carried forward.

The fetal envelope was tough and but slightly adherent to the outer envelope. There was no difficulty in separating the uterus from the mass in front, and it was noticed, as enucleation was carried forward, that the musculoperitoneal covering before referred to disappeared everywhere as a covering of the sac, except upon the anterior and extreme left surface and upon the upper and free surface of the tumor. The tumor behind was surrounded by agglutinated intestine and omentum. Upon the right side of the sac there was a similar covering of adventitious tissue attached to the colon and cecum. In front the musculoperitoneal covering was continuous with the left horn of the uterus. The uterus occupied a position in front of the sac to the right of the median line and was separated from the sac by adventitious tissue only, and the lower portion of the sac seemed to rest upon the pelvic tissue; so we had to deal with a case of tubal gestation which ruptured at eleven weeks, the products of gestation escaping into the pelvic cavity and there finding lodgement and attachment. It retained its vitality and went on developing as a tubo-pelvo-abdominal pregnancy.

During the process of enucleation we accidentally ruptured the sac, and a small amount of fluid escaped, so we deemed it wise to remove the fetus, when we continued enucleation, removing the entire sac.

In lifting up the sac from the pelvis a portion remained adherent and was torn off. It was afterward stripped from its attachment. There was no frightful hemorrhage, but a considerable amount of oozing from the bottom and sides of the pelvis. This not being checked by hot sponge-packing, it was deemed advisable to pack the cavity with iodoform gauze, bringing one end out of the lower angle of the incision.

As we reached this stage of the operation the anesthetist reported that the patient had collapsed suddenly, and we were compelled to suspend operating for a few moments to assist in restoring the

patient. The means we employed were the subcutaneous transfusion of eight ounces of normal salt solution, a hypodermatic injection of strychnine, and lowering the head.

In our haste to close the abdomen we neglected to stitch the sac to the incision, and this was the cause for some anxiety, our fear being that so large a raw surface was left that we would be likely later to encounter obstruction of the bowels through intestinal adhesions. Happily our fears have not been realized. The patient was put to bed in fair condition and made an uninterrupted recovery, leaving the hospital at the end of the fifth week.

The proper treatment of ectopic pregnancy after the sixth month, when the child is still living, is still under discussion. This paper is written for the purpose of considering one phase only of the subject, viz., Shall operative intervention be instituted during the period of viability of the child, or shall we await the death of the child and the cessation of the active circulation of the placenta and then operate?

The settlement of this question must hinge upon the relative mortality to the mother of the different procedures. The ectopic infant is of such low vitality and so frequently deformed that if to rescue it greatly jeopardizes the life of the mother, then ethically one must withhold the hand and permit it to die that the greater life may be saved.

Such have been the sentiments of the writer for many years. Until within the last few months all published statistics the writer has been able to study have shown a larger percentage of recoveries to mothers when the fetus was allowed to die and the mother operated upon later. The tables of Bland Sutton in the last edition[1] of his book clearly point to the greater safety of this method.

The writer has had three cases of ectopic gestation at or near full term. In the first the operation was done during spurious labor at full term. In this case, although fetal movements were detected one hour before operation, the child never breathed after removal by abdominal incision. The mother died the eighth day after operation, of hemorrhage resulting from the removal of the placenta, which was left at the time of the initial operation. The

[1] "Surgical Diseases of the Ovaries and Fallopian Tubes, including Tubal Gestation," pp. 315, 317.

placenta was removed thus early in consequence of impending death from septicemia. This patient was operated upon at the City Hospital, September 14, 1895. A detailed history of the case was reported to the Marion County Medical Society, and at the same time the specimen was exhibited.

The second case was one operated upon five months after spurious labor occurring at full term. The fetal movements ceased at the time of spurious labor. The fetus and sac were completely removed through an abdominal incision and the mother made a good recovery. This case was reported in a paper published in *The American Journal of Obstetrics*, vol. xxxvi., No. 1, 1897. The third case was the one the history of which I have read today.

The writer felt very well satisfied with the results in his last case until he saw, later, Harris's table in Kelly's recent work and Ayers' table in the February (1899) number of *Obstetrics*. These tables show clearly the decrease in mortality of late years when the operation is done with a viable fetus.

The tables are, however, both incomplete, as all tables purporting to be a summary of the world's history of work done in any particular line, especially when they comprise so long a period of time as that included in the two tables referred to. As evidence of the incompleteness of these tables, compare the tables of Harris and Ayers for the years 1894, 1895, 1896.

Harris tabulates 13 cases with 3 maternal deaths and 10 maternal recoveries when the operation was done during the viability of the fetus. For the same years Ayers tabulates 7 cases not included in Harris's tables ; of these 7 cases there were 3 maternal deaths and 4 recoveries.

The writer of this paper has collected from reliable sources 5 other cases reported during the same period and operated upon under similar conditions. Of these 5 cases, 4 mothers died and 1 recovered. By combining the tables we have reported 25 cases in all operated upon during the three years mentioned. Fifteen mothers recovered and 10 mothers died, the percentage of recoveries of mothers being 60.

Again, in Ayers' table for the years 1894, 1895, 1896 is given a list of 16 cases operated upon at varying periods after the death of the fetus. Of these cases 10 mothers died and 6 recovered.

The writer has to add to this list 17 other cases reported during the same years. Of these 17 cases, 4 mothers died and 13 recovered. We have, then, by combining these tables, in all 33 cases, in which 19 mothers recovered and 14 died, thus making the percentage of recoveries of mothers 57.7 (see tables published herewith).

It seems to the writer that by combining these tables we have as near an approach to accuracy as is possible in gathering statistics from journals of all countries and languages.

If the tables do approximately show the percentage of recoveries to mothers, then we have here an answer to the perplexing and heretofore mooted question, Shall operative intervention be instituted during the life of the child or only after its death? We are not compelled to longer consider and weigh the probability of the length of life in days of the child; the safety of the mother alone demands intervention. Undoubtedly other questions will urge themselves upon us, such, for instance, as, Is it less dangerous to operate at six months or at nine months, and what treatment of the placenta yields the least mortality?

These are still under debate, but it is aside from the writer's purpose to discuss them in this paper. His only endeavor has been to strive to arrive at a correct and truthful answer of the question, Is it safer for the mother to be operated upon in a case of ectopic gestation during the viability of the fetus? He believes that the statistics he has collected and compiled are so nearly complete and correct that the answer may now be given unqualifiedly in the affirmative.

There will be undoubtedly individual cases in which good judgment of the surgeon will direct him to await the death of the fetus. These will be exceptional cases. The rule will be to operate at or near term during the life of the child.

TABLE I.—TUBAL GESTATION.

Celiotomy between six and nine months, child being alive.

Operator.	Date of pregnancy.	Treatment of placenta.	Mother.	Reference.
Cripps	6 months.	Left.	Died.	British Med. Journ., London, 1896, p. 779.
Dunning[1] . . .	Term.	Left.	Died.	Unpublished.
W. R. Wilson .	Term.	Left.	Died.	Amer. Journ. Gyn. and Obstet., vol. iii. p. 605.
A. J. McCosh .	6½ months.	Removed.	Recovered.	Med. and Surg. Report, Presbyt. Hosp., New York, 1896, p. 112.
W. F. McNutt .	Term.	Removed.	Died.	Journ. Amer. Med. Assoc., vol. xiii. p. 278.

TABLE II.

Abdominal section after death of fetus, at or near term.

Operator.	Fetus dead.	Treatment of placenta.	Results.	Reference.
S. W. Budd . .	4 months.	.....	Died.	Va. Med. Monthly, vol. xx. pp. 957 to 962.
S. W. Budd . .	14 days.	Removed.	Recovered.	Ibid.
J. Philips . . .	2½ months.	Removed.	Recovered.	Trans. London Obst. Soc., 1894, vol. xxxv. p. 162.
J. M. Rector . .	3 months.	Removed.	Died.	New York Med. Journ., vol lx. p. 648.
H. Meek . . .	?	Removed.	Recovered.	Amer. Journ. Obst., vol. xxix. p. 84.
J. W. Elliott . .	?	Removed.	Recovered.	Boston Med and Surg. Journ., vol. cxxx. p. 260.
Vignard . . .	?	?	Recovered.	Amer. Journ. Obst., September, 1894.
J. A. Shaw-Mackenzie	?	Removed.	Recovered.	British Gyn. Journ., London, 1894-95, vol. x. p. 341.
McClintock . .	8 months.	Removed.	Recovered.	Kansas Med. Journ., Topeka, vol. vii. p. 15.
E. K. Ballard .	6–7 months.	Placenta absorbed.	Died.	Amer. Journ. Obst., vol. xxxiii. p. 714.
H. F. Bigger . .	Term unknown.	Placenta absorbed.	Recovered.	N. Amer. Journ. Homœopathy, 1894, vol. lx. p. 768.
C F. A. Moses .	4 months.	Removed.	Recovered.	Edinburgh Med. Journ., vol. xi. p. 421.
R. Ludlum. .	Some time.	Left	Recovered.	Clinique, Chicago, vol. xv. p. 471.
J. D Griffith .	18 months.	Removed.	Recovered.	Trans. Med. Association, Mo., pp. 299-301.
F. Michinard .	Not stated.	Removed.	Recovered.	Amer. Journ. Obst., July, 1896, p. 36.
C. G. Franklin .	Term child; not stated how long dead.	Removed; and sac stitched.	Died.	British Med. Journ., vol. l. p. 1819.
A. H. F. Barbour	Not stated; 6 months fetus developed.	Removed.	Died.	Edinburgh Med. Journ., 1894 and 1895, vol. xl. p. 211.

[1] Reported to the Marion County Medical Society, November, 1895.

SUMMARY OF CASES REPORTED IN THE YEARS 1894, 1895, 1896.

Fetus alive at time of operation.

	No. of cases.	*Mothers recovered.*	*Percentage.*
From Harris's table[1]	13	10	
From Ayers' table[2]	7	4	
From Dunning's table[3]	5	1	
Total	25	15	60 per ct.

Fetus dead at time of operation.

From Ayers' table	16	6	
From Dunning's table	17	13	
Total	33	19	57.7 per ct.

DISCUSSION.

DR. J. HENRY CARSTENS, of Detroit.—I fully agree with the essayist that the proper way, other things being equal, is to operate when the child is living and viable, and to do so at once. As far as the operation is concerned, I should say the case was a favorable one. The placenta is not always attached to the broad ligament, and it does not grow down in the folds of the broad ligament. Very often it can be enucleated. In some instances rupture takes place, and the placenta is found attached directly to the intestine, the colon, the rectum, or the sigmoid, and in such a case it is utterly impossible to remove the placenta in operating when the child is alive. I have seen such operations performed by others, and the patients have invariably died from what I considered hemorrhage; they called it shock. When you have a case to deal with in which the peritoneal covering of the broad ligament becomes attached to the intestine, to the colon, or any other part, then you can shell it out. You can separate the peritoneal adhesions, shell out the whole sac, and then you have simply the broad ligament as a pedicle to deal with.

DR. RICKETTS.—At what month, Doctor?

DR. CARSTENS.—Any month, if you have that kind of a case; but if there is rupture, and the placenta is attached to the intestine, it is utterly impossible to remove it.

I reported a case a year and a half ago where I operated after the

[1] Kelly's Operative Gynecology, vol. ii. p. 459.
[2] Obstetrics, February, 1899.
[3] Accompanying table.

child died; it was a great deal like Dr. Dunning's case in many respects. The child had been dead for some time, it having lived about seven and a half months. The broad ligament was split and became attached to the sigmoid, so that it was easy to separate it and to enucleate it. But in other cases, where this cannot be done, there is only one way to do it, and that is to stitch the sac to the abdominal wall, and do not touch the placenta. Leave it alone. I remember one such case in which it took two weeks before the placenta came away entirely. However, in such a case there is very little danger if we do not meddle with the placenta. But the great tendency with the *general* surgeon is to remove things and do something in order to earn his money. I think there are times when it is a great deal better for us not to earn our money, and let our patients live.

Dr. Lewis S. McMurtry, of Louisville.—By referring to the proceedings of the Association at the Pittsburg meeting last year, you will discover that we had quite a free and extensive discussion on this subject, and I believe Dr. Werder presented a specimen at the time. I simply rise to indorse what Dr. Carstens has said and to make one qualification of his remarks. Dr. Carstens struck the key-note in determining the course to pursue in dealing with these cases. By referring to one of the earlier volumes of the Association you will find a case reported by me where the fetus had gone on to full term, was dead, and some weeks after a spurious labor, when there were beginning septic symptoms, I operated. No fetal movements had been observed for nearly three weeks. The placenta was very large; there was no sign of any atrophic changes in it in consequence of the death of the fetus. It was spread out over one side of the uterus, over the colon and ileum, and there was a fearful and profuse hemorrhage following an attempt to enucleate the placenta, something that will never be seen at the operating-table in any other condition. It is perfectly dreadful. The woman is exsanguinated inside of a minute; there is complete collapse, the pelvis is full of blood, and that is the end of it. When you have the placenta disposed toward the broad ligament and uterus, as in the case Dr. Dunning has described, you have a good opportunity for complete enucleation. Dr. Carstens alluded to leaving the placenta, and I have no doubt it is much more preferable than to attempt to enucleate it in such cases; but those cases, a great many of them, will die of hemorrhage. In Dr. Price's case, which he reported at a previous meeting, I believe death occurred on the eighth day after the operation, from hemorrhage, the placenta having been left behind. Other patients will die of sepsis. You are all familiar with Tait's

method of dealing with the sac by stitching it to the abdominal wall and attempting to treat it so as to avoid this accident; but I do not think it has been efficacious in a great number of cases. When we come down to the practical point it is about this: When the placenta is distributed in the manner Dr. Dunning has described and Dr. Carstens has emphasized in connection with the broad ligament, you can enucleate it and get a good result; but if it is spread out over the intestines and you have to dissect it out, then you will have a hemorrhage that will be fatal.

DR. X. O. WERDER, of Pittsburg, Pa.—I am very sorry that I did not hear all of Dr. Dunning's paper, because I am particularly interested in the subject and have had a little experience with this class of cases. I have reported in the TRANSACTIONS of this Association (I believe it was at the Toronto meeting) one case in which I did a laparatomy for ectopic gestation at or within two weeks of full term, in which I succeeded in removing the placenta and fully two-thirds of the sac. The rest of the sac was adherent to the intestines, and I found it advisable to stitch it to the abdominal wall. The hemorrhage during the removal of the placenta was profuse, but I succeeded in controlling it promptly by clamping the uterine and ovarian arteries. The patient made a good recovery, and the child lived four days.

Nearly two years ago I had another case of ectopic gestation at full term. The woman came to me at about the seventh month. I kept her under observation at the hospital for several weeks, because she was feeble, anemic, and a broken-down young woman. I operated about a month before the end of term, believing that by so doing the child would not be as much crippled as if it were allowed to grow any longer. I opened the abdomen and found the fetus in the sac. It was partly an intraligamentous tumor, but evidently the ligaments had become ruptured, and the largest portion of the sac was in the general abdominal cavity. I dealt with it the same as I would deal with an intraligamentous or firmly adherent cyst. Without opening the sac I broke up the adhesions, tied the bleeding points, and complimented myself that I would have no trouble. But I unfortunately perforated the sac at one point, and just at that time the patient began to strain, as she was not completely anesthetized, and a portion of the fetal extremities —a hand or foot, I do not remember which—pushed through the opening. The patient still continuing to strain, all at once the fetus was extruded through the opening in the sac, followed by one of the most frightful hemorrhages I have ever seen in my life. Only a few seconds had elapsed and the abdomen was literally filled with blood. I grabbed with one hand half a dozen towels lying in a basin, and

with them compressed the placenta with all the strength I had. This controlled the bleeding temporarily; then my assistant compressed the abdominal aorta, and under this pressure I rapidly removed the placenta without much hemorrhage. There was not much bleeding of any consequence after that. I enucleated the sac completely, removed the uterus, which was firmly adherent to the sac, and left the patient in as good a condition as many patients are after a severe abdominal section. The pulse was probably 140, but in spite of stimulation it did not improve. She died on the third day, with the pulse ranging from 140 to 170 during that time. I believe it was probably due as much to shock as hemorrhage.

I wish to say, in regard to the remarks of Dr. Carstens, that if we can remove the placenta and sac we should do so by all means. It is the only surgical method of dealing with these cases. Leaving the placenta to slough for a month or six months is all nonsense, when we can remove it with our improved surgical technique. We will succeed in removing both placenta and sac just as we succeed in dealing with the pedicle in hysterectomy with an improved surgical technique. Ten or twelve years ago we heard in discussions that it was impossible to deal with the uterine stump the same as we deal with the stump of an ovarian cystoma. In fifteen years from now we will not speak of leaving the placenta and sac after an operation for ectopic gestation.

Dr. Carstens referred to the placenta being attached to the bowels, and in that case leaving it in the abdomen. I believe at the present time that that is probably the wisest course to pursue. Can we tell whether it is attached to the bowels or not? We do not know what the placenta is attached to until we get in there and begin to enucleate the sac. My idea would be to begin enucleating the sac, and of course we do not know what we are going to encounter and to what the placenta is attached until we get to it. The best plan of treatment is to tie off the vessels before we begin to do anything with the sac. If we can get at the uterine and ovarian arteries and clamp them, we can then begin the enucleation with comparative safety.

Dr. Ricketts.—Was that done in the case in which there was a fatal result?

Dr. Werder.—No; it was not done because I could not get at them. The fetal sac blocked the pelvis completely, so that it was impossible to get at any of the arteries. This evidently cannot be done in all cases. It is a subject about which we have to learn a good deal. I believe in the course of time we will learn and know better how to deal with the placenta than we do now.

DR. DAVIS.—Why not compress the aorta at the beginning of the enucleation and keep the finger there until you are through?

DR. WERDER.—As soon as we have hemorrhage from the placenta the aorta should be compressed and the placenta removed. Speaking of sloughing of the placenta, if you leave the placenta, pack the cavity with iodoform gauze. How else does the placenta come away?

DR. CARSTENS.—The placenta does not slough; it comes away in pieces. It becomes necrotic. It gradually softens down and liquefies; it is kept aseptic. You do not have sloughing of the placenta, and there is no septic infection at all; it comes away like the retained menstrual blood in the uterus if it is aseptic.

DR. WERDER.—Some of them may come away in that manner. I have read reports in which operators have described the odor as horrible, and I believe it is a hard matter to keep a sac of that kind aseptic for six weeks before the placenta comes away.

DR. JOSEPH EASTMAN, of Indianapolis (by invitation).—I have had a little experience in dealing with ectopic gestation. I recall the features of a case upon which I operated some ten years ago, and they are as vividly in my mind to-day as ever. One of the speakers referred to a case in which he encountered a terrible hemorrhage. I can fully corroborate what he said in regard to that point. I never saw such a terrible hemorrhage as in the case I refer to. The gentleman said that the blood simply pours out, and the patient is exsanguinated in a few moments. That is the case, provided the placenta is being detached. The patient upon whom I operated had an enormous mass in the abdomen. We made the ordinary incision, and were seeking gently to determine our bearings in the case when I discovered that a large portion of the sac in front was completely gangrenous, so that this patient had not only a live child inside of her, but a dead sac. There was no amniotic fluid around the child, not even a teacupful in which the child could float. By gentle manipulation the gangrenous portion gave way, and we had at once a detached placenta, with all the horrors of blood pouring out. We threw hot pieces of gauze about it, and this seemed to detach it more, and the blood came more abundantly. Here is a point which is of decided value and familiar to all of you: Try and find out where the uterus is—and in these cases it is very important to be able to find the cornu of the uterus, from which the tube has sprung and which contains the child. I immediately plunged my left hand down to find the uterus, and found myself grasping a mass as large as my hat. I could scarcely encircle the neck of the sac as it emerged from the uterus with my fingers. In doing so I found I was detaching the placenta and getting still more

hemorrhage. The nurse and physicians who were mopping up the blood could scarcely get enough away to see the margins of the wound. A clamp which I had invented for ovarian pedicles, which narrows the broad and rounds the flat, happened to be within reach. By the aid of this clamp I succeeded in expressing the placenta two-thirds out of the sac and had control of the hemorrhage below. The child was still lying in there, the placenta being partly in front of the child. After having controlled the hemorrhage I drew a long breath, removed the child and placenta, and then began to deliberately separate formidable adhesions. It is my belief that we may meet with one case in which the placenta can be shelled out nicely, while in the other it is exceedingly difficult. I may yet be in a corner where I shall have to leave the placenta to slough away, but I hope I never shall, providing I have opportunity to seize the cornu of the uterus quickly. It must be done quickly, or the patient will lose so much blood that she cannot recover. With the aid of first finding the cornu of the uterus and applying this clamp, no matter how great the hemorrhage, you will have stopped it. You may tie it off; you may quilt it with silk or use the serre-neud. Every operator, after controlling the hemorrhage, can separate any adhesions which he may find to the greater omentum or intestines, or to any part in connection with them. In this case I had the gratification at the end of nine months of having the patient's photograph with a live child in her arms. The child died soon after. The father came home, took it for a long buggy ride on the street; it took cold, and died of pneumonia. But this had nothing to do with the operation. The mother is still alive and well. She weighs two hundred pounds, and is a rosy-cheeked woman. I repeat again, that the keynote to all is to immediately find the cornu of the uterus, and have something at hand with which to seize it. You cannot fumble around to find the uterine or ovarian arteries, as there is time lost in doing this. Something must be done instantly. During this operation, while considering the matter, I recalled an expression by Mr. Lawson Tait, given in his book, namely, that "the golden rule is not to touch the placenta." That rule, like Lawson Tait himself, is dead.

DR. JOHN M. DUFF, of Pittsburg.—I have had only a limited experience in operating on extrauterine pregnancy at term, having simply assisted others, but I desire to refer to one point for fear that some of the remarks that have been made may be misinterpreted by some. I do not think we should take much consideration of the life of the child. It is not possible to remove the placenta in all cases in operating on a woman while the child is living. I do not think that should be considered

notwithstanding the brilliant results of Dr. Eastman and the fact that a few others have been able to deliver a living child at term. Few of them are delivered alive, and those that are delivered alive had better be dead, because most of them die soon afterward.

It was my privilege to be present at Dr. Werder's operation, but unfortunately I did not see his manipulation of the placenta, because he had asked me to take charge of the child. I got the child to breathe, but it died of pneumonia. I do not think we ought to be misled with the idea of allowing ectopic gestation to go on until viability of the child with the hope of saving it. While my remarks are perhaps a little foreign to the essential points of the discussion, I believe in every case of extrauterine pregnancy, as soon as the diagnosis is established, arrangements should be made to perform an operation.

Dr. D. Tod Gilliam, of Columbus, Ohio.—Six or seven years ago I read a paper before the Ohio State Medical Society in which I advocated the removal of the placenta in every case of extrauterine pregnancy. I tried to show at that time that it was absolutely impossible for the placenta to have any vascular connection with any part except from the natural source—the utero-ovarian system. I established what I considered a beautiful and incontrovertible theory. I had four cases of viable children up to that time, and had removed the placenta in each case successfully. The first thing, after I went home from the meeting, was a call to Lancaster, Ohio, to see a case of extrauterine pregnancy, and I thought of doing the same thing as I had done before, which I did, and rapidly lost my patient. I believe now that there are cases in which we cannot remove the placenta, in which we will have to do the best thing possible. As Dr. Hall said (he read a paper on the same subject at the same meeting): "Whichever way you deal with the placenta, you will wish before you get through that you had done it the other way." There are cases in which no rule will apply. I believe in the majority of cases, if we get the uterine cornu we will control the greatest amount of blood supply. We have had some recent light thrown on the anatomy of these parts. It is believed that the ovary and the tube receive their blood supply as the ovarian artery comes in from the pelvic wall. We now know that the ovarian artery makes its way to the side of the uterus and there meets with the uterine artery, and that recurrent branches supply these parts. In ectopic pregnancy, if we go down on the uterine side and clamp the vessels we will command a large portion of the circulation, although if the vascular attachments on the outside are numerous we may not be able to get enough of it to save our patient. There may be a vascularity on the outside sufficient to exsanguinate and kill

the patient. The time will come when we shall be able to deal with these cases to better advantage than we do now. We will take out the uterus in most cases where the child is viable; we will go down, and wherever we find vessels clamp them, and thus control the circulation connected with the uterus, because it is the starting-point from which the vascular supply must come. If we can get hold of the vessels we will succeed in many cases in successfully removing the fetus and its envelopes.

Dr. Edwin Ricketts, of Cincinnati, Ohio.—A few years since the question of handling extrauterine pregnancy sufficiently was about as important as dealing with hemorrhage in this class of cases. There is one thing I want to say a few words about, and that is, in an extrauterine pregnancy, where the placenta is out among the intestines, I do not care at what stage you begin, after the abdomen is opened you commence to tear off the attached placenta. Now, the peripheral margin of the placenta should not be attacked first. Apply forceps wherever possible *over* the uterine supply, and then, where it is possible to do so, seize the ovarian artery, and you are at liberty to go ahead and shell out the placenta. I have seen cases of extrauterine pregnancy die from loss of hemorrhage by not paying attention to this plan.

I wish to refer to a case of delay which came under my care last April, and which was largely the result of neglect. The child was dead. An abscess had formed some time in January, and in April, when the abdomen was opened, we found the placenta had been split one-third into the uterus. In order to save the life of the patient we had to do a hysterectomy. The uterus was shelled out, *appendicitis* was found, and a *five-inch opening in the large intestine* which required *fifteen interrupted sutures*. I speak of this to show the folly of delay. The patient recovered after a hard fight of five weeks. The point I want to lay special stress upon is this: That in going down after the placenta it is foolish to try to dig away the peripheral margin first, when you have the source of blood supply back here (illustrating by diagram). On the other hand, according to the manner in which you shell out the uterus, the tube may be torn close off to the uterus. You use the clamp or some other device to get around the uterus in order to control the hemorrhage in the manner alluded to by Dr. Eastman. It is in such cases that generally we need an instrument which will enable us to grasp the uterus in such a manner as to control hemorrhage. With cases at full term the first thing to be considered is to get at the *source of blood supply*, and do the turning out afterward.

Dr. James F. W. Ross, of Toronto, Canada.—Since our last meet-

ing I have had just such an unfortunate case as Dr. Werder's, and when the blood poured out in such a large quantity I thought of the interesting case he reported at a previous meeting. My case was one in which pregnancy had been diagnosticated, but there were no symptoms of ectopic gestation. I myself did not think the case was one of ectopic pregnancy. I determined that the uterus was empty, and that there was a mass behind the uterus in the cul-de-sac of Douglas. I operated, began to enucleate the sac, found it stretched across to the right side of the pelvis, and blood began to pour out as the placenta separated from the bottom. It was, however, in the abdominal cavity, attached to the branches of the large veins in the pelvis. The bleeding was so profuse that I packed with towels, put on clamps, first on one broad ligament, being careful not to injure the ureter, and finding that the hemorrhage did not cease I rapidly clamped the other (right) broad ligament. This stopped the hemorrhage to a great extent. I thought it was desirable to do a hysterectomy. I removed the sac; the patient lost much blood, and died a day or two afterward, either from the excessive loss of blood or from shock—I do not know exactly which. These cases, to my mind, should not be dealt with as early as they are diagnosticated, and I do not agree with Dr. Duff in that regard. I believe after the case reaches the fourth month it would be easier to operate at full term than to operate at the fourth or fifth month. The sac is thin, the placenta is more definitely detachable, and it is less dangerous than operating at the fifth or sixth month of pregnancy.

There is one point Dr. Dunning did not mention in referring to the danger of these cases—that is, secondary rupture of the sac. I met with one case at full term in which secondary rupture had occurred, with the escape of the liquor amnii into the abdominal cavity, so that the woman nearly died. A good sized drainage-tube was introduced, the cavity washed out twice a day regularly to keep away the grumous discharge of which Dr. Carstens spoke. It has been recommended by an English operator to remove the child, close the sac, wait for secondary septic symptoms, and again open it. Mr. Tait discussed this question at the time, and said it would not be a bad plan so long as the surgeon reopened when septic symptoms occurred. In this way the placenta could be kept aseptic, if such a thing is possible.

Another class of cases is that in which we come down upon the placenta, at first in front, if the sac is opened. Dr. Werder's plan of enucleating the sac is much preferable to that of making a direct incision into the sac itself and delivering the child the way we do in a Cesarean section. I have seen two cases in which the placenta was

removed from among the intestines. Tait removed the placenta from among the intestines in one case, and the hemorrhage was frightful. I saw another case in which this was done, and the patient died shortly afterward. How to control the hemorrhage in these cases is a difficult problem to solve. It is true, pressure of the abdominal aorta may do so, but the large sinuses on the wall of the intestine pour forth the blood, and packing will assist in controlling hemorrhage to a certain extent.

As to the method of Dr. Ricketts, referred to, where we put the clamps on the placenta and tie it off as we would the omentum, it is something that ought to be tried. Perhaps at some future time we may be able to tie off the placenta just as we now tie off the omentum and drop back the remainder.

DR. DUNNING (closing the discussion).—I have very little to say. The discussion has taken a direction that I did not expect it to do. I only considered one question in the paper, and that was, in cases of ectopic pregnancy at or near full term, shall we proceed to operate, or allow the child to die, and then operate? I did not discuss methods. I am glad, however, that methods have been discussed, because I have decided views upon some phases of this subject. I believe we ought to leave the placenta except where it is attached above, or else where it is included entirely in the broad ligament. In dealing with the placenta we have not reached a method which is not open to objections, so that frequently we will resort to one method in one case and to another in another case, on account of the variety of anatomical conditions to be found. For instance, we may have a form of ectopic pregnancy which is retroperitoneal, where the placenta is attached above and never disturbed, or it may be attached to the interior of the Fallopian tube, and in a case of that sort we can tie the horn of the uterus and begin at once to enucleate without any hemorrhage whatever. In cases in which we have rupture of the tube, and the placenta is extruded at once or gradually, the vascular supply and attachments will be so great as to render the operation exceedingly difficult, but I did not intend to discuss this phase of the subject at all. The only one question I brought forward has already been considered. My reason for bringing this question before the Association is this: That in all of our recent text-books, with but one or two exceptions so far as I am acquainted with them, we are advised to wait for the death of the child before we operate where we find the patient just at the close of the period of pregnancy. This is the advice given by Bland Sutton; it is the advice in substance given by Greig Smith; it is the advice given by Lusk in his article in Coe's

book. He says, however, it looks as though we had reached the time when we should not fold our hands and wait for the child to die. There is a uniformity of opinion amongst text-book writers upon this subject, unless it be that Howard Kelly makes an exception and recommends operation with the viability of the child. I hold a different view to-day than I did when I operated on my case in January. My views have been changed in consequence of a statistical study of the subject. The tables which have been presented to us heretofore have been faulty and exceedingly misleading, particularly those presented by Bland Sutton. They are the most elaborate and most convincing of any tables heretofore presented, and it was to refute the idea of Bland Sutton that I wrote this paper. I thank you very much for the attention you have given me.

WHAT SHALL WE DO WITH THE POST-OPERATIVE HEMORRHAGE OF CELIOTOMY?

By D. TOD GILLIAM, M.D.,
COLUMBUS.

"If the patient, having rallied from her ether, with a good pulse and practically normal temperature, be found in the course of the next twenty-four hours to be showing the indications of collapse, together with a rising pulse and falling temperature, hemorrhage will almost always be found to be at the bottom of the trouble. The pulse under these circumstances becomes feeble and is rapid and running in character. The temperature and pulse, together with the general condition of lassitude and growing indifference, are almost pathognomonic of the condition. If the bleeding be allowed to continue these symptoms gradually deepen, and the more advanced indications of collapse, such as great pallor, sighing, and cold surface, supervene. . . . The symptoms of shock may readily be mistaken for hemorrhage, the difference being that in hemorrhage the indications do not begin for some hours after operation, while in shock they are present from the first. Otherwise the two present so many points of likeness that it is at times difficult to say which is present."

The above, taken from one of our leading text-books, is a very fair exposé of the prevailing knowledge regarding the symptomatology of internal or concealed hemorrhage. I believe it to be practically correct. Still I must say that there are few questions onnected with gynecic surgery that have given me more concern and upon which I have been more at sea than the one under consideration. Could we always be sure of our diagnosis, matters would be much simplified ; but even this would not solve it, for we have to deal with other factors, such as the source and amount of hemorrhage and our ability to control it by other than surgical measures.

To make a long story short, I will say that my experience with the post-operative hemorrhage of celiotomy and conditions simulating the same has not been of a character to confirm me in my earlier and more radical views as to treatment. Time was when I could have answered this question with alacrity, when I should have smiled pityingly on the man that hesitated. My motto was: "Open up and secure the bleeding vessels." I believed that all the dictates of reason and conscience demanded it, that surgical instinct demanded it. I believed then, as many believe now, that surgical instinct was the synonym for bold, active interference. Thanks to the tempering influence of age and experience, I have attained a higher conception of the term and its significance. As I stood by the bedside of the patient in whom I suspected internal hemorrhage, surgical instinct whispered: "Open up and seek for the bleeding vessels." When, some hours later, I looked upon the lifeless form of this self-same patient and witnessed the outcome of my uncalled-for interference, I began to question the identity of my monitor. A little later and I stood at the bedside of an apoplectic, and I asked myself why surgical instinct did not prompt me to lift the calvarium and go after the bleeding vessel. Then it dawned upon me that surgical instinct no more implies active surgical interference than practical medicine implies active purgation. True surgical instinct conserves the best interests of the patient.

As has been said, the diagnosis, the prognosis, the natural tendency, and the amenability to treatment all play a rôle in the management of these cases. That the diagnosis is sometimes beset with difficulties insurmountable is the testimony of all experienced observers. The subnormal temperature, which, in connection with the rapid pulse, blanched surface, and great prostration, is depended upon as being specially significant of hemorrhage, may, and sometimes does, depend on something else. It is a physiological fact that the vessels of the portal system alone, when distended, are capable of containing all, or nearly all, the blood of the body; that when so distended they form a reservoir into which may be drained the blood of the brain, the vital centres, and of the heart itself. It is well known that patients sometimes perish from the accumulation of blood in the enormously distended vessels of this region. It is known, furthermore, that certain

vasomotor disturbances are responsible for this condition. When such is the case the temperature falls, the pulse runs riot, and all the indications point to an internal hemorrhage; and while there has not been the loss of one drop of the vital fluid, this diversion serves to kill as quickly and effectually as though a trunk vessel had been severed. These are the cases of which authors speak as bleeding to death in their own veins. There are other conditions that influence the temperature, the exact nature of which we cannot determine, but which nevertheless produce a symptomatology so essentially like that of internal hemorrhage as to be indistinguishable from it. Some of these conditions are illustrated in the following cases.

The first was a case of bleeding fibroid for which I did hysterectomy. The patient was greatly exsanguinated, nervous, and depressed. The operation was without incident, and before closing her up it was noticed that the pelvis was exceptionally dry. This was the first of a series of four abdominal operations for that morning, and owing to the very favorable trend of affairs, and the excellent condition of the patient when lifted from the table, one of the assistants remarked that he believed the patient could walk down town. Four hours later I was summoned to the hospital and found the patient in collapse with the characteristic symptoms of internal hemorrhage. Examination revealed about four ounces of blood in the vagina. She was lifted on the table, and with an aneurism needle two stout ligatures were introduced, one on either side of the cervix, and firmly tied. Fearing, however, that internal hemorrhage might be going on, I made hasty preparation and opened the abdomen. I found about two ounces of blood in the pelvis, every ligature in place, and not a sign of hemorrhage. She was closed up and returned to bed, but, despite all efforts, sank and expired four hours later from shock. It may be contended that the loss of six ounces of blood was sufficient to bring about the fatal issue in this delicate and bloodless woman, but I do not believe it, for she had been habitually losing much more at short intervals, with little appreciable effect except to keep her pale and weak.

But, lest there should be some misgivings on this score, I will cite another case, about which there can be no question. This was a delicate and neurotic woman on whom I had performed

hysterectomy for a troublesome fibroid of small size. The patient left the table in good condition. A few hours later I was summoned to the hospital and found her collapsed. She had every indication of internal hemorrhage. I pursued the same tactics as in the preceding case: first tying the vessels on either side of the cervix, and then opening the abdomen. There was no blood in the vagina and none in the abdomen; nevertheless she gradually sank, and expired within twenty-four hours, never having rallied from the shock.

Some time after this, and while the painful remembrance of these cases still haunted me, I had an experience after ovariotomy that in many of its features tallied closely with the last-described case. I contented myself in this case in using such restoratives as are usually resorted to in shock, such as strychnine hypodermatically, hot applications to the surface, elevation of the foot of the bed, stimulating rectal injections, and hypodermoclysis of the normal salt solution, all of which had been used in the preceding cases; and to my great relief, after several hours of anxious effort, I had the satisfaction of seeing my patient well out of danger. I firmly believe that both of my first patients would have rallied and probably survived the operation had I not opened the peritoneal cavity. I could add to this list two other cases with a fatal issue in which internal hemorrhage was strongly suspected, but in one of which a post-mortem by myself revealed a clean abdomen, and in the other, according to the statement of the embalmer, there was no evidence of hemorrhage. I could also adduce several additional instances in which, profound shock coming on several hours after operation and characterized by rapid pulse and subnormal temperature, the patients rallied and recovered.

As will be seen from the above, the presently received indications of internal hemorrhage are not infallible, and, if implicitly relied on, may lead to grave mistakes, as in two of the cases cited above. I do not deny the great value of these diagnostic criteria, nor that as a rule they may be relied on, but the numerous exceptions, taken in conjunction with other features yet to be developed, should make us chary of precipitate surgical interference.

The prognosis of internal hemorrhage is another factor that should claim our attention. In the earlier days of my abdominal work, when the drainage-tube constituted a necessary and fre-

quently utilized part of my armamentarium, I had numerous instances of quite free internal hemorrhage, but which recovered without surgical intervention. One case in particular gave me great concern, and I was several times on the point of radical interference, but, happily for the patient and myself, refrained. The patient was a single lady, about thirty years of age, the sister of an army officer. I had removed the appendages. Soon after the operation blood began to appear in the tube in inordinate quantities, so that, at intervals of half an hour to an hour, from one to three ounces of blood were withdrawn. This continued several days, and the patient's pulse mounted to 135, accompanied with marked prostration. At this juncture, and as a last resort before the use of the knife, I gave ergotol hypodermatically and the hemorrhage ceased. But the most notable case of internal hemorrhage succeeding operation, with ocular proof and spontaneous cessation, that has ever come under my observation, was that of a minister's wife upon whom double ovariotomy had been performed by a colleague assisted by myself. The operation was done in the morning, and at 2 o'clock the following morning I was called for by the doctor, saying that his patient was bleeding. Arrived at the house, I found that the blood had forced itself through the incision and had saturated the dressings and bedding. She was lying in a pool of blood. My colleague gave the anesthetic, and, by the dim light of a smoking lamp held by the reverend husband, I proceeded to open her up. I found a large quantity of blood in the cavity. The patient was at no time fully under the anesthetic, and for the most part very much alive, and when the husband observed me bailing out the abdomen with my hands, he exclaimed: "She cannot live; she will surely die." To which she responded: "Oh, no, George; I will not die." Later, as I was bringing up the stumps to examine the ligatures, she complained bitterly of pain in the hypochondrium corresponding to the pedicle being handled. I mention this as an illustration of reflected pain. Examination of the ligatures showed them to be intact and not a drop of blood escaping from the pedicles. I was not able to determine the source of hemorrhage, and, after completing the toilet of the peritoneum as best I could with the means at hand, I closed her up. Several days subsequently she died of peritonitis without a recurrence of hemorrhage. Thus

it will be seen that, with ocular proof of profuse internal hemorrhage, the prognosis is not always bad, even where the case is left to nature.

Aside from the diagnosis and prognosis, which, as has been seen, are often obscure and sometimes misleading, there are other considerations which cause us to hesitate before resorting to surgical intervention in cases of suspected hemorrhages following celiotomy. I refer to the inherently dangerous character of the intervention. If one were to look up the statistics on this subject, I feel assured that the death-rate would be found appallingly high—that it would far exceed that of the recoveries. Now, this occurs not only in the practice of the untrained, but in the hands of the most careful and expert among us; hence I say that the work is inherently dangerous. If it were only a question of making section in suspected intraperitoneal hemorrhage, I should not hesitate long; or even if it involved the reopening of the wound immediately or soon after its closure, I should not greatly fear the issue; but when it comes to the reopening of a wound that has been closed for several hours, I confess to an unconquerable aversion. It is not because we cannot be as clean, as careful, as in the primary operation; it is because of an essential difference in the attendant danger. Why this is I have not been able to make out, but suspect that it depends largely on the fact that the vital energies have been pretty well exhausted at the primary operation. It would seem that following the primary operation, especially after the lapse of a few hours, all the reparative machinery of the economy has been put in operation; that there is a marshalling and massing of all the vital forces for a supreme effort. If you go in there now you carry confusion into the ranks of your allies and cripple the resources of the economy to a most dangerous degree. Doubtless the time at which such secondary operations are performed has much to do with the result. Owing to the obscure symptomatology there is little or nothing to indicate the presence of internal hemorrhage until evidences of shock are so pronounced as to force themselves on the attention of the nurse. In the majority of instances the surgeon, who has done his morning's work, is far away busily engaged in the daily routine which follows his hospital duties. By the time he is found and arrives at the bedside of the patient the shock has

deepened into collapse and the patient is *in extremis*. He takes desperate chances for a desperate condition, and as a rule the patient succumbs.

In conclusion, I have little to offer in the way of suggestion. As a burnt child dreads the fire, so my most painful experience in the tragic cases cited above has imbued me with a wholesome dread of delayed interference after celiotomy.

This, of course, refers to intraperitoneal interference, for I have on several occasions opened up the abdominal incision down to the peritoneum for hemorrhage from the walls, without untoward result. This latter class of cases can usually be easily distinguished from intraperitoneal bleeding by the puffed and discolored appearance of the tissues along the line of incision. If I were satisfied that a large vessel had let go, as indicated by the rapid development of symptoms indicative of hemorrhage, I should go into that abdomen with the utmost celerity. But this paper was not inspired by any hope or expectation of being able to suggest any line of action, my sole object being to elicit an expression of the prevailing views of my Fellows. Still, I would like to make one suggestion with reference to the medical treatment of such cases, and that is in the use of atropine. Some time since I had in charge a young lady who was subject to the most violent and persistent uterine hemorrhage of unaccountable origin. She had passed through many hands before coming to me, and had tried many of the vaunted remedies without avail (as she could not make up her mind to curettage), when in one of her spells she fell into the hands of my brother, Dr. Charles F. Gilliam. He placed her on atropine, with the result that, after the physiological effects of the atropine became manifest, the bleeding ceased. Since then her attacks have been less frequent and always promptly amenable to the atropine treatment. Other cases followed in his and my practice, among which were some intractable cases that had been curetted, and in every instance so far the hemorrhage has been controlled by the atropine. I am nothing of a therapeutist, and in fact am something of a therapeutic nihilist, but the results have been so convincing in the cases that have fallen under my observation as to force conviction. As to the *modus operandi*, I can only speculate. We know that atropine increases the cutaneous circulation, producing a general and marked hyperemia of the

surface; we know that the cutaneous vessels are capable of containing nearly one-half of the blood of the body, hence by derivation it diminishes the amount of blood circulating in the internal organs. It is not altogether improbable, indeed, I think it quite likely, that the vasomotor action that dilates the cutaneous vessels coincidently and by way of equation constringes the visceral vessels. The duodenal ulcers resulting from extensive burns of the skin would argue in favor of compensatory vascularity. It is not expected that this or any other medicinal agent will arrest the torrential hemorrhage of the larger vessels, such as the uterine or ovarian arteries, but is especially applicable to that troublesome form of hemorrhage which emanates from numerous vessels of smaller caliber. As to the vital question, when we shall interfere surgically and when refrain, this paper ends where it began—in an interrogation point?

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DISCUSSION.

Dr. Charles A. L. Reed, of Cincinnati, Ohio.—I have rarely listened to a paper so well written and excellently read as the one just contributed by our confrère from Columbus. I plead guilty of the same sense of dread in reopening the abdominal cavity, and I regret that he closed his paper as he did, with an interrogation point, rather than an exclamation point, following the word "Eureka!" Because if there is one thing for which I pray more than another, it is some safe rule for our guidance under these circumstances. Things were different when we used to put a drainage-tube in every case, because we could then see what was going on, particularly if any hemorrhage was taking place, and this stands a potent argument in favor of drainage. I do not employ drainage as frequently as I formerly did, and I think there are outweighing considerations. The discrimination between shock and hemorrhage is not in any manner or means an easy one, particularly in the case in hand, because we are liable to be dealing with both conditions. Indeed, purely surgical shock as a single factor in the cause of death is, to my mind, the rarest possible occurrence. I must confess that where I observe symptoms unmistakably indicative of internal hemorrhage I want to get at the bleeding-point, and I want it quick. I recall some cases in which I have to deal with post-operative hemorrhage following laparatomy. I recall a case that was sent to me some years ago by my neighbor, Dr.

Fackler, from whom I removed extensively diseased appendages and enormously distended pus-tubes. I put on my ligature satisfactorily; put the patient to bed in such a good condition that I hesitated to put in a drainage-tube. Fortunately, I did insert a drainage-tube, and presently the nurse began pumping out considerable quantities of blood. The patient began to show loss of blood. I reopened the abdomen, found the pedicles perfectly satisfactory, but in the course of the enucleation I wounded a branch of the mesenteric artery away up under the mesocolon, a circumstance which had eluded my attention at the time of the operation. The woman rallied without untoward symptoms; there was no extreme exacerbation of shock; no great prostration, and my patient seemed to get along as well as if the accident had not occurred.

I recall another case that happened during my experience at the Cincinnati Hospital, in which the patient, in a moment of delirium, immediately after operation jumped out of bed, the result of which was a hemorrhage. Whether it was this violent action on the part of the patient that produced displacement of the ligature or not I do not know, or whether it was some fault of mine in its original application. At any rate, the ligature had slipped and the patient was a victim of hemorrhage. I happened at the time to be out of town—I had gone to a hospital somewhat distant—and my interne called me by telephone and reported the condition the woman was in. He asked what should be done under the circumstances, and whether it was necessary to summon another surgeon. He reopened the abdomen himself, tied the pedicle which had slipped, and saved the patient.

On the other hand, I recall the most melancholy case that has happened in my experience for a long time—one that occurred last year. The patient came from Kentucky, for whom I extirpated the uterus, doing a panhysterectomy. I closed the operative wound in the peritoneal cavity very carefully; it was absolutely dry; there was no appearance of hemorrhage whatever, and the patient was in splendid condition. I closed the abdomen entirely, went about my work, did not visit the patient that evening, went out of town the next morning, and shortly after she was dead. There was an insidious hemorrhage in this case, as was revealed by post-mortem examination, from the posterior vaginal artery, which simply kept leaking away until she was practically *in extremis*. Hypodermoclysis was resorted to without avail. I felt sure that if the condition had been detected in time and I had been summoned early the patient would have been saved. For one, I am not disposed to trust to symptoms of

bleeding, yet I recognize their confusing character when there are certain conditions which may mask that of a leaking vessel.

As to the therapeutics of this question, it is very important. I have noticed in the literature that there is a tendency, on account of the failing powers of the patient in one of these leaking vessels, to pump in strychnine. I do not think it is advisable to do this, because the strychnine is apt to increase the hemorrhage. I look upon hypodermoclysis as exceedingly important.

Dr. H. W. Longyear, of Detroit, Mich.—The subject raised by the paper is certainly one that interests all of us who do abdominal surgery. There is nothing more puzzling to me than to know just what to do in cases which show this condition of collapse, which has been described by the doctor as indicative of internal hemorrhage; and yet there are other conditions which produce much the same symptoms, and thus it is necessary to use fine discrimination, so that unnecessary operative work may not be done. If we know there is a bleeding vessel it is our duty to reopen the abdomen and tie the vessel. It makes little difference, then, what condition the patient is in; the line of duty is plain, just as in a case of ruptured tubal pregnancy, when we know there is internal hemorrhage taking place, it is our duty to reopen the wound and do the best we can. But if we do not carefully analyze the symptoms we will not always find hemorrhage present when we open up. I have had experience with two cases in the last two years which brings this forcibly to my mind. In one I reopened and found no hemorrhage; the woman died soon afterward. Post-mortem examination revealed an antemortem heart-clot, which was evidently the cause of death. In the other case, simply a curettement, the patient died with the same symptoms. Post-mortem examination showed an antemortem heart-clot. What causes that condition is a question. I have seen the theory advanced somewhere (I do not remember just where now) that there is some change produced in the intima of the bloodvessels by the anesthetic, which allows coagulation of the blood within the blood-channels. This may possibly be the explanation. Certainly a case which dies after a curettage would indicate that something of that kind was the cause of death. We have to make our examinations with these points in view before we determine to open the abdomen.

Dr. Rufus B. Hall, of Cincinnati, Ohio.—I did not have the pleasure of hearing all of this paper, but it occurred to me in listening to the discussion, that in many of these cases the question of hemorrhage or shock can be determined largely by the operator himself. We are pretty well aware when we put a patient to bed after an

operation whether there is great danger or not of her having a secondary hemorrhage. If the operation has been a complicated one, if there has been a condition left where we might have great risk of hemorrhage, this would aid us a great deal in determining whether or not the patient was suffering from hemorrhage. I fully agree with one of the speakers in saying that patients die from surgical shock but very seldom. My way of spelling shock is hemorrhage; yet I am aware that patients may die from shock. But I would not hesitate a moment to take out a stitch, if the abdomen has been closed without drainage, and without very extensive exploration we could determine readily whether there was hemorrhage going on or not. If there is no hemorrhage it adds very little to the risk by this. If hemorrhage is taking place, it is our duty to give the patient a chance for her life by tying the bleeding vessel. It is true that some of them will die, but others will not. If you allow the hemorrhage to go on until the woman has lost a large quantity of blood she will surely die. In my judgment, much depends upon prompt interference in these cases. I would reopen the abdomen of every one of them just as early after I discovered the hemorrhage as possible. I would give them a chance for their lives by so doing that they could not have otherwise. The main object is to obtain the sentiment of the Fellows as to what is the correct thing to do. When we are certain that there is a bleeding vessel we should reopen, seek for the bleeding-point, and tie it.

Dr. Lewis S. McMurtry, of Louisville, Ky.—I do not think that the surgeon can be found, after having done very many abdominal sections, who has not had a post-operative hemorrhage from slipping of the pedicle. But I desire to call the attention of the Fellows to the fact that this is very much less frequent than it was formerly. In the early days, before our operative technique was as much improved as it is to-day, the drainage-tube was in universal use, and it was common to see twenty-four or forty-eight hours after operation great quantities of blood pumped out of the drainage-tube. Hemostasis was not as thorough then as it is now. Surgeons did not include the small vessels. Now, these are ligated carefully. The fan-shaped pedicle tied *en masse* is a most unsurgical procedure. Vessels should be tied separately, and whenever possible the ovarian artery should be secured well over to the side of the pelvis in operations for removal of the uterine appendages, and ligatures should not be left where it can be helped. I have pursued the course of securing the ends of vessels with small ligature, which gives additional security. The method of securing vessels, and the method of hemostasis, and the details of operative procedures are so much better to-day than they were for-

merly, that these accidents do not occur so frequently in the practice of those who have had an extensive experience.

Dr. J. Henry Carstens, of Detroit, Mich.—It is not my desire to speak on every subject, but this matter is so important that I am compelled to say a few words. Last week I had two cases of sarcoma of the ovary. The woman did not have much shock when placed in bed. She had a pulse of 74, and the next time it was taken, an hour or an hour and a half later, it was 78. When I reached the hospital two hours later she had no pulse that was perceptible at the wrist. She was in collapse. I did not reopen her abdomen and search for a bleeding vessel, because I knew there was no blood there. It was simply a case of shock, and the whole question is one of diagnosis. If I put my patient to bed, and at the end of an hour or two the pulse is 110, and then suddenly drops, it is not hemorrhage. Hemorrhage comes on with a gradually increasing pulse. We can diagnosticate the case without any trouble. As Dr. McMurtry has said, if we follow the modern improved surgical technique, and tie each vessel separately, we will very rarely have post-operative hemorrhage. Formerly, as he says, we used to have many cases of post-operative hemorrhage.

As to medication, I believe in the efficacy of it, and I think atropine or belladonna is a good thing. I would use either of them in preference to strychnine. I would use it to stimulate the sympathetic system.

Dr. James F. W. Ross, of Toronto, Canada.—Just a word or two with reference to this subject. There are two classes of hemorrhage—hemorrhage which occurs shortly after operation and hemorrhage which occurs from three or four to five days subsequent to operation. It has been my lot in a couple of pus cases in which a drainage-tube was left, after a violent attack of retching, occurring several days after operation, to have a hemorrhage. I have remained with the patient two hours or more, observing the condition carefully, hesitating whether to reopen the abdomen or not. In another case I reopened, and in each case the patient made a complete recovery.

In cases of thick pus-tubes, with a very edematous pedicle, we are liable to have hemorrhage from the pedicle and a rotten condition of the tissues afterward.

Regarding immediate hemorrhage, I want to emphasize what Dr. Hall has said. We are too much afraid of making a little opening in the abdomen. If we have a case in which we have a hematoma of the ovary that has ruptured and flooded the peritoneal cavity with its contents, we are liable to have sepsis occurring within a short period of time. With the sepsis we may have some elevation of temperature,

but at any rate a rapid acceleration of pulse, and it is therefore necessary to diagnosticate between septic infection and hemorrhage. To do so we can take out a stitch and introduce a flat-ended, long silver probe in different directions after it has been thoroughly sterilized, and if there is blood it will escape and show itself. I have had one that thus stimulated hemorrhage, in which I did not reopen the abdomen of the patient, and she died of peritonitis. No hemorrhage was found. In another case I found hemorrhage, tied the bleeding vessel, and the woman recovered.

DR. L. H. DUNNING, of Indianapolis, Ind.—I rise to call attention to a valuable diagnostic sign in differentiating between shock and hemorrhage. I am forced to believe in shock in some instances. I have seen it occur in twelve, twenty-four, and thirty-six hours after operation. I have seen patients recover from it who would not have recovered from hemorrhage. This diagnostic sign is an irregular capillary circulation or congestion here and there. I recall three or four cases in which I have stood for hours at the bedside of patients hesitating between shock and hemorrhage, and on the appearance of localized spots I have decided it was shock. I did not reopen the abdomen in these cases, and the woman recovered. A purple ear or purple lip, a spot occurring upon the face, is a diagnostic point of great value between shock and hemorrhage. If you have a purple ear you know you do not have internal hemorrhage. It is shock, and you can best use non-operative measures for relief.

DR. X. O. WERDER, of Pittsburg, Pa.—I wish to call attention to some observations in cases of this kind. There is one rule, and it is this: Patients suffering from secondary hemorrhage, as Dr. Gilliam has stated, have a rapid pulse, lowered temperature, restlessness, etc. On two occasions I have seen evidences of returning hemorrhage five or six hours after operation with a rise of temperature. At the time the abdomen was reopened the temperature was over 102°, pulse 140 or more. I believe, therefore, that the temperature is of no great consequence, and should not interfere with our reopening the abdomen. The pulse, in my opinion, is a very important indicator. As Dr. Carstens has said, if there is a gradual rise of the pulse you have a hemorrhage, no matter what the temperature may be. Another important sign is the respiration. In a patient who has internal hemorrhage the respirations will increase in the same proportion as the pulse increases. Those two symptoms—a rapid and increasing pulse, with a rapid increasing respiration—are the most important signs.

In regard to the treatment, the sooner the abdomen of the patient is reopened in these cases the better. I would not even think it necessary

to give an anesthetic. My rule is, as soon as I suspect an internal hemorrhage, to take out a stitch, run my finger around the cavity, and I invariably find my diagnosis confirmed. Then I reopen the abdominal wound and tie the bleeding point without an anesthetic. The patients will stand the operation very well without being anesthetized. The patients will not suffer very much by this interference without anesthesia. In one instance the patient survived the operation; other patients lived for three or four days and died of sepsis.

Dr. Edwin Ricketts, of Cincinnati, Ohio.—Whenever ligatures are made use of there is one procedure that I desire to refer to, one that is always resorted to by Bantock—that is, crushing the tissues for half or one-quarter of a minute with a heavy jaw clamp, and after the removal of the clamp the ligature is applied. The angiotribe, which is being advocated a good deal by some operators, in which the ligature is not used at all, is permitted to remain for two and a half minutes, and the appendages are removed. It is claimed that there is no hemorrhage following its use. The plan suggested by Bantock, however, is one that should not be lost sight of in applying the ligature.

Dr. William H. Humiston, of Cleveland, Ohio.—Dr. McMurtry has hit the nail on the head, and I agree with him that it is unnecessary to have a secondary hemorrhage, provided the surgeon takes proper care of the bleeding vessels during the operation. I do not believe in the use of the *en masse* ligature for the appendages. Each vessel should be caught separately and tied with fine catgut or silk, re-enforced by securing the ovarian artery as it comes from the pelvic wall. It is not necessary to crush the tissues after the manner mentioned by Dr. Ricketts. If you do, it leaves the dead product there, which nature must take care of, and there is great danger of sepsis following.

Dr. Gilliam (closing the discussion).—I am gratified at the free discussion which my paper has elicited from the Fellows. As I stated, my object was not to impart information on this subject but to obtain it, and I must confess that I have received a great deal of benefit from this discussion. The question remains, however, that there are cases in which we will be much in doubt as to whether we have hemorrhage or not. By the Hall method of opening up the wound and introducing the finger we can tell whether we have hemorrhage, but we cannot tell the amount of it. Blood in the drainage-tube is not a criterion of a dangerous hemorrhage. The amount of blood in the drainage-tube is no certain index of the activity of the hemorrhage or the amount of blood in the peritoneal cavity. One may insert the tip of the index finger into the abdominal cavity and obtain blood where there is but

very little bleeding going on. But when we have cases in which the blood wells up through the wound it is an entirely different matter.

Then, again, where the abdominal wound is closed by continuous suture in tiers you cannot take out a single stitch. When you break the continuity of the suture you might as well open the wound in its entire length. This you will have to do, and it is not a trivial matter.

The suggestion made by Dr. Dunning as to the irregularity of the capillary circulation is something new to me. I shall look for it hereafter with a great deal of interest. The fact remains that opening the abdominal wound a certain number of hours after a hysterectomy is dangerous. We cannot be sure whether we have a dangerous or a life-destroying hemorrhage taking place. If we do not reopen the abdomen in these cases the patients are liable to die, and if we do they die, but there is a possibility of saving lives by so doing. The mortality arising from reopening the abdomen is appalling, and I hope we shall be able to get something more definite to follow in dealing with this class of cases.

I am thankful to the Fellows for the manner in which they have considered this subject. I have received some new points, but there is yet very much for us to learn.

HOUSE-TO-HOUSE OPERATING.

By EDWIN RICKETTS, M.D.,
CINCINNATI.

THE skilled abdominal and gynecological surgeon of today is a product of surgical evolution, to which no man ever gave such an impetus as the great and lamented Mr. Lawson Tait. His lines in surgical work were so simple, so thorough, so rapid, and withal so exacting on himself, assistants, nurses, and patients, for pushing rapidly the case to a successful issue or to the best possible result, that we should never lose sight of them, lest we forget that simplicity, rapidity, rigid cleanliness, and thoroughness are to be our keynotes of success. Tait began his work in his private house, without trained nurses, and even then had no mean death-rate, and gradually lowered the same markedly after training his assistants and nurses to his own fashion. After all of this experience he resorted to house-to-house operating, a most fitting close of his life's work in surgical diseases of women, and food for sober thought, lest we are tempted to go off on a tangent by falling in line with the fad of the day—that of chemical antiseptics, as is only too often attempted to be practised in many of our hospitals.

The fresh air of the country homes, and their hygienic environments, are most important factors that come to our aid in doing house-to-house surgery. I am ready to acknowledge that the surgeon in our line of work, as in others, is improving, and I stand ready to make the same claim for the general practitioner in his ability to care for the after-treatment, assisted by the nurse, aided by the telephone, telegraph, and by rapid transit if your presence is needed.

There is the mental dread of having to go into an institution which in many patients brings on post-operative disasters, that are undeniably eliminated by operating in their own homes; there is a love of home developed under just such trying circumstances that

should not be underestimated. How often do we see patients expressing a desire, after being operated upon in a hospital, either public or private, for something seemingly trivial from home, and how matters are helped if the desire is fulfilled!

Iron bedsteads, with other improved household furniture, are found in many rural homes as permanencies, and yet they are not an absolute necessity for any patient who has to undergo a surgical operation. The late Dr. Alexander Dunlap did four hundred ovariotomies, with a death-rate of 15 per cent., never putting one of his patients on an iron bed; he never saw our modern operating-tables with their thousands of improvements, for the greater part of his work was of the pioneer kind. These operating-tables, giving all planes, from standing a patient on his head to the reverse position, found in so many "diving-tank" operating-rooms, are not superior in results obtained to those of the kitchen table found in any house of the land. Nor is the showy cleaning of the patient ever to be countenanced, especially while that patient is under anesthesia; such work should be done most thoroughly and most carefully before anesthesia is begun.

The robing of operator, assistants, and nurses in rubber suits and fishing boots, while the audience composing the "outer circle" is looked upon as teeming with dangerous and detestable microbes, is truly degrading to our art; it lowers the dignity and usefulness of our profession, and is anything but a scientific show. This "outer circle" soon makes up its mind, and justly, too, that surely it is not so dirty, for the infected cases still follow, notwithstanding the great crying of these would-be "holier than thou" surgeons. Treves has put it well in saying that "such is more allied to a fervent idolatrous ritual brought down to the level of popular performance." The modern, smooth-faced, rubber-mailed surgeon, of rubber gloves, face mask, boots, hat, and coat, is truly a sight to behold, but not one for emulation, either from a scientific or a practical stand-point. He is a walking, self-instigated Turkish bath; and how the dirt with the epitheliated microbes—anal, and possibly urethral and vaginal as well—must pour down, mixed with perspiration, into the rubber wells of his boots! This perspiring is certainly not a cleanly process, but it goes on right in the midst of operating, and is promoted and hastened by the wearing of these rubber articles.

My house-to-house work is done on the kitchen or dining-room table in each house, properly arranged by the nurse sent ahead the day before I operate. The doctors invited are not asked if they have taken a bath. They come into the room when we are ready for them, look on, touch nothing in any way connected with the operation, and retire as soon as the patient is put to bed. Frequently some of the relatives desire to witness the operation, and when it is possible you gain by allowing them to do so.

We have no sterilizer to make a display in cleaning instruments or dressings; no baked glass jars for holding dressings, simply the original package; no foot jiggers to cause the hot and the cold water to flow; no glass-topped operating-table; no dressing forceps, save those that nature gave us; no instruments resting in hot water or in antiseptic solution; no blue, red, or white water impregnated with chemicals to produce necrotic tissue; but after washing the abdomen thoroughly with soap and water, then likewise our hands, we apply freely 98 per cent. alcohol and then begin our work, using towels that have been dried in the sun and ironed by the help at hand. My results have been all that the most fastidious could desire; and of the houses of different construction that I have operated in, the old log-house stands supreme for cleanliness, much to the credit of its occupants, and this same cleanliness was all that I asked for.

Richelot, in a recent article "On the Relative Value of Antisepsis and Improved Technique" before the International Gynecological Congress, says that "the very best antiseptic does not retain its value for clinical purposes, and laboratory prognoses are not always to be relied upon. It is found that the use of antiseptics is not only inefficient, but at times dangerous. Hence, they have gradually become more or less discredited, whilst sterilization by heat is daily gaining favor." He further says: "The best results are obtained by a surgeon who knows how to use his hands and his common-sense. Methods continue to become more and more simplified, and in that way lies progress."

Bumm, on the same subject, says: "The absolute reliance on the protective power of antiseptics has been much diminished by exact investigations, as it has been proved by a whole series of experiments and observations that the elimination of all microorganisms during the operation has not been attained; there is no

way to remove with certitude all micro-organisms from the hands; it is therefore illusory to think that either asepsis or antisepsis can bring about a sterile condition of the wound. In spite of the proved presence of micro-organisms, most wounds heal without suppuration or fever. In addition to this, short operations do not so much tax the resisting power of the wounded tissues as well as the entire organism. To expose the peritoneal cavity during a long time has a well-known bad influence on the heart, the intestines, and the serosa. Asepsis has delivered us of the dangerous application of too many concentrated disinfectants. Antiseptics and improved technique have to go hand-in-hand—the drier the wound the better are the chances for primary healing." This last statement I want to impress upon you as being a surgical fact recognized for centuries.

The tendency in medicine and surgery is to claim that many individual views are new when the author has been dead for years and years. Let us never fail to honor the memory of those to whom honor is due.

Thorough, rapid operating, under rules of simple cleanliness, will continue to produce more and better results than are possible with the strictest rules of antiseptics; slow, tedious operating, carried out with hesitancy, is always to be condemned.

House-to-house operating means house-to-house isolation, and for this the room-to-room risks of infection in a hospital are lessened to a minimum degree. Surgical cleanliness is surgical godliness, and every time we forget this we add to our death-rate. Chemical perfumes of various kinds will *never* displace soap and hot water judiciously applied in surgery. A strange cat in a strange garret is uncomfortable; and add to this the fact of his being sick, something he cannot help, and you increase his misery. So it is with the majority of the patients operated upon outside of their homes. The average rural home location is not bad, and with scrubbing brush, water, and soap it can be continually made to shine out for the demonstration of surgical truths that are Gibraltars to us and are the indwellings of the people, who have long since learned to love and cherish these in sickness as well as in health. Our best surgery can be given them here, and our reward is to be the lowest death-rate possible.

DISCUSSION.

Dr. Walter B. Dorsett, of St. Louis, Mo.—In order to obtain success in surgical operations we should try to make things as simple as possible, and the word simplicity in surgical work carries with it a great weight. The greatest of men are known by their simple way of expressing themselves, and the greatest of surgeons are known by their simple way of operating. The essayist has made an effort not to discard antiseptics or antiseptic methods, as I understand it, but rather to do away with a great many things that are supposed to be superfluous. In the consideration of the subject before us there are many points that should not be overlooked in regard to the advisability of isolating patients from "house to house," as he expressed it. In the first place, we cannot have the discipline maintained in a private house that we can in a hospital, whether we have trained nurses or not, because the relatives of patients are always coming in and out, and they undoubtedly carry germs with them which have a certain amount of influence over the patient. The nervous system of the patient is more or less disturbed, and recovery is in a great measure retarded. On the other hand, I am willing to admit that in hospitals a great many things are done that should not be done. A great deal of the work of cleansing the hands by different methods I regard as entirely unnecessary. I use bichloride of mercury in cleansing my hands, washing them first with soap and water. I have used carbolic acid and creolin; I have never used lime. I have heard some gentlemen say they have used lime and soda afterward. I use permanganate of potassium and oxalic acid with soap and water. There is harm done by the bichloride pack on the patient. I know of instances in which I am sure non-closure of the wound and the healing process were materially retarded by the bichloride pack. I believe green soap, and plenty of it, and good, hot water are all that is necessary.

The essayist ridiculed the idea of putting on different dressings, or rather the manner in which the surgeon should dress himself. A rubber apron to protect his clothes and a sterilized gown over it is all that is necessary. I believe a cap should be worn. The hatband the surgeon wears around in summer and winter is certainly infected with germs, and if one is doing an abdominal operation and perspiration falls from the forehead into the wound, it necessarily carries microorganisms into the abdominal cavity.

With reference to the matter of flushing, we all concede that too much of it has heretofore been done. Septic material in the cul-de-

sac of Douglas has been washed up under the liver and a larger area of the peritoneum has been immersed with septic material and bad results have followed. Swabbing out of material is all that is necessary. In regard to the use of water on the wound, I suppose he means by that the abdominal incision. Dry dressings are always to be preferred, consisting of sterilized boric acid. I never take my bandage off under eight or ten days, and it is seldom I have a stitch-hole abscess, and that is due to the fact that I use dry dressings almost entirely.

Dr. J. Henry Carstens, of Detroit.—I am sorry that the paper of Dr. Ricketts was read. It is wrong in principle to teach something that ought not to be taught. This paper will be read by a great many general practitioners who will simply laugh at aseptic and antiseptic surgery, and they will say: "What is the use; it doesn't do any good anyhow?" I hold it is wrong to promulgate such doctrines, and Dr. Ricketts knows it. Dr. Ricketts would not have the mortality of Dunlap of 15 per cent. by operating in a well-equipped hospital with all the facilities at hand. If he had anywhere near this mortality from house-to-house operating he would be ashamed of himself; he would lose his practice in time. It would hardly do for us in these days with an improved technique to have a mortality of 15 per cent. following the removal of pus-tubes, etc. He quotes Tait, who is known to have written articles repeatedly against antiseptic surgery, and those articles have had an injurious effect all over the world. Dozens of women have died on account of lack of aseptic precautions, because surgeons have taken Tait for an example. There is either something in bacteriology or there is not. If Dr. Ricketts does not believe in bacteriology, or does not believe in proper disinfection, does not believe in anything, but teaches the doctrine of cleanliness, I say, all right; let him go ahead. If anyone believes in bacteria, it is very important that they should use some agent or agents with which to limit their spread or drive them out of the tissues. Dr. Ricketts does not dare tell us that the general country practitioner, who is a nice, good, honest man, who sometimes works in the garden, takes care of his horses, etc., has as good success in operating on patients as the city surgeon who does his work in a well-equipped hospital, where he has all the facilities for doing such work. It is impossible for any man to get as good results from house-to-house operating as he can in a private hospital. He has not got the assistance; he has not got an experienced man, as a rule, who knows how to administer the anesthetic. He has not got in many instances good water. In a great many of the towns it is really necessary to boil the water before it is

fit to drink. In a well-equipped hospital, with assistance, you can do operations in ten minutes where it would take thirty or more minutes in a country house. There is less danger of opening the abdominal cavity. The patient herself is not better in her own home; she is better in a hospital. If she is operated upon at her home, the neighbors and friends, her sisters, mother, uncles, cousins, and aunts are constantly coming in, so that the patient is made much worse by it. The mother or a sister of the patient may suggest that the patient has a pain in her stomach, a bachache, or a headache, and finally she thinks she has a headache. Now, if such a patient is in a hospital under a trained nurse, she never thinks about those things. She goes to sleep, and remains quiet until the next morning. If the relatives are allowed to come around, as they are usually in a private house, they may make a fuss and keep the patient awake. A private house is not the place in which to perform an operation, except in cases of emergency. Of course, a cervix or perineal operation may be done in a private home; but when it comes to performing large operations it is better to have patients transferred to a hospital where you have all the facilities for operating. Your success will be greater. There will not be a mortality of 15 per cent. I want to deprecate and most emphatically protest against belittling of aseptic precautions, because it will have a demoralizing effect on the whole profession. Some general practitioners will believe, after reading a paper like this, that there is nothing in asepsis, and they will say to themselves, if there is nothing in it, what is the use of taking so much care and pains; we had better keep on in the old way.

Dr. William H. Humiston, of Cleveland, Ohio.—If we were to take Dr. Ricketts seriously I think we would be taking a backward step of fifteen years. I cannot believe that he is serious in many points he has made. Anyone who has seen the late Mr. Tait operate knows that his technique was the perfection of cleanliness. Bantock is also a man who talks a great deal against antiseptics, and yet he is one of the cleanest operators one can see. He believes in absolute cleanliness, and so did Mr. Tait. I prefer to have my patients in a hospital, where there is less annoyance than in a private home. I recall one case in which the patient had a nervous chill on the second day after the operation, due to the cook coming into her room and making many complaints, ending by threatening to leave. The nurse you take with you to a private house will not always work in harmony with the servants, and *vice versa*. Another bad feature is the numerous visitors who will flock in and weary the patient, often bringing unsuitable food and drinks, and urging the patient to take them, thus producing

indigestion, vomiting, etc. This would not occur if the patient was in a hospital. It is possible to do an abdominal operation in a hospital, and care for the patient in a general ward and not dress the case until the eighth or tenth day, and obtain primary union, although there are typhoid fever and tuberculous cases in that ward. I have demonstrated the possibility of this a number of times in the past three years. Nearly all of my clinical cases are operated upon in the general amphitheatre before many students, and then placed in a general ward for recovery. I have now four patients convalescing in the ward of Charity Hospital where there is a case of typhoid fever, one of tuberculosis, and one of pneumonia. The wound will not be exposed until union is secured, and we are ready to remove the sutures at about the tenth day.

Dr. Willis G. Macdonald, of Albany, N. Y.—It is very disagreeable for me, as the Fellows can readily appreciate, to disagree with my friend, Dr. Ricketts. He has said a great many things which I have not listened to with very much comfort. In the first place, he is mistaken when he speaks of the presence of antiseptic surgeons and antiseptic surgery. The day of the purely antiseptic surgeon and of antiseptic surgery has now passed for at least ten years. There are few men who are doing much surgery in this country who begin to make use of chemical agents for the purpose of sterilization to the extent that they did fifteen years ago. The system of operating has entirely changed in that respect; that, I believe, is generally conceded by all of us. Out of a considerable experience, beginning from the same suggestions that he did, following the technique of Mr. Tait and the methods of work by Mr. Bantock, in hospitals and in private houses, we have a death-rate which was far beyond what it should have been. If our present system of operative surgery is correct, if it is based upon the evidence of micro-organisms, then the most painstaking technique which can be devised, the most careful training, should be resorted to. I want to say right here that it is impossible to train men to step into an operating-room and be in a condition to stand beside the operating-table in the course of a few months. It takes considerable time, and they require extra training which they should receive in the bacteriological laboratory. I am not disposed to disagree with Dr. Ricketts in saying that a good surgeon can operate in almost any house where there is a likelihood of his having sterilized water to use about his patient, his own hands, etc. It is true, the surgeon may send his nurse on a day or two before the operation, and the water may be boiled and ready for use; but there are few operations I have seen undertaken under such circumstances where the water was not so hot that it was impossible for one to put his hand

in it, and where one's technique all goes to pieces, so to speak, because cold water from the tap is generally put in to cool the water. It is just here that he might have boiled the water in the beginning.

While much that Dr. Ricketts has said appeals to our old notions and excites our sympathy, etc., yet, after all, it is not good surgery, and never will be. Personally, I would no more undertake, excepting in an emergency, the operation of hysterectomy in a private house than I would think of doing a resection of the stomach, because I should feel reasonably certain that my patient would assume at least ten times as many chances to lose her life as she would in a well-equipped hospital. There is no occasion for it. I find that the people of the State of New York are coming to appreciate what a good hospital with all its facilities really means. They appreciate what a good training school for nurses means. They appreciate that they can be made very much more comfortable, and that their chances of recovery are immeasurably increased by going to hospitals to be operated upon instead of remaining at home; and even granting that the mortality is not greater, that the chances of infection are not greater in a private house, I do not see that my friend Ricketts has presented anything to you that you can take away from here as a guide toward conducting operations in private houses.

It is not possible to thoroughly sterilize the hands by any method which is well recognized. Again, sterilization is not always infallible, particularly so far as bacteriological tests go, and I believe they are fairly complete. I never knew a patient to die of sepsis but that a careful bacteriologic examination would give the reason why. Different methods of sterilization yield different results. I myself have never been an advocate of the use of permanganate of potassium or of the oxalic method. In connection with our clinical work we have made more than a thousand observations, conducted in the bacteriologic laboratory, with reference to the matter of cleaning the hands, because every now and then we have slight degrees of sepsis, a little weeping from wounds, showing that somebody's hands were not quite sterilized. Our bacteriologist takes a culture from everybody's hands who takes an active part in the operation, and usually in the first trip around he will find one pair of hands that are not sterile. By simply letting down a little bit in the technique trouble is very liable to follow in the way of sepsis. I regret exceedingly that a certain amount of surgery must of necessity be done from house to house. We are compelled to operate at times in a certain class of acute cases, such as hernia, intestinal obstruction, extrauterine pregnancy, where very little can be done in the way of preparation. We have reached that degree

of refinement in our section of the country that we carry our cold water with us, taken from the sterilizing-room. We do not expect to use more than four quarts, and this quantity we take with us. We have an instrument sterilizer which we take with us for the purpose of boiling our instruments. We have a metal cartridge-box, of the capacity of perhaps a little more than a cubic foot, which will contain two sterilized aprons, two caps, two pairs of cotton gloves, half a dozen towels, a gauze sheet, a set of gauze sponges, two large gauze abdominal tampons, abdominal dressing and binder, and then there is a tube which contains sufficient silkworm-gut for the ligatures of any ordinary operation. It contains a dozen sterilized Lembert sutures, threaded. There is a package of silver-foil. This contains a little cylinder that slips inside of the sterilizer. The instruments are sterilized at fifteen pounds pressure for fifteen minutes. The box is put in a leather container, and it is only open at the time the operation is begun. The container holds the instruments or utensils for cleansing our hands. It contains a Kelly pad, rubber sheets, which are thrown into a 1 : 1000 bichloride of mercury solution, which we generally use; and in that way we carry with us these two packages, which weigh perhaps sixty pounds, and we are able to do in cases of emergency fairly decent surgery. Even in the matter of minor pelvic surgery, repairing a lacerated cervix or perineum, I do not like to go too deeply into the tissues about the vagina in a private house, and then leave the case to the care of perhaps an unskilful nurse. I have seen a good deal of suppuration occur in these wounds at times.

Dr. Rufus B. Hall, of Cincinnati.—I would defend the essayist if he had confined his remarks to emergency surgery, and I should not have taken issue with him on the subject. I believe there is a certain amount of emergency surgery which must be done and ought to be done in private houses, in a hotel, or in a boarding-house, in any place that the patient happens to be. We should not, however, make these places our choice for operations. I want to emphasize the remarks of one of the previous speakers by saying that it is wrong for the patient and for the profession to have it go out from this Association that we indorse the sentiments expressed in this paper. I believe it would set us back to the time of Dr. Dunlap, when he had a mortality of 20 per cent. from his abdominal operations. This applies equally well to operations for ovarian cysts. Dr. Dunlap was a pioneer, and we honor his name and memory; but if there was a surgeon today having a 20 per cent. mortality, he would not operate very long if it was generally known. Patients would not be operated on by such a man if they knew he had such a high mortality following his operations. We

know that there are a certain number of cases which we encounter in abdominal surgery in which it is difficult to kill them, and they will get well in spite of anything we may do. I am willing to admit that these operations should be made under the best surroundings. It is true, we go into the country and operate upon emergency cases of ectopic pregnancy, appendicitis with suppuration, gunshot and stab wounds, etc., and get fairly good results; but deliberately to go into the country and do these operations in farm-houses, in people's residences, as a matter of choice, is a mistake. I do not care to do it, and whenever it is possible I would prefer operating on my patients in a well-equipped hospital. If for no other reason than the after-care of the patient, I would refuse to do such operations in the country. The greatest risk is the increased mortality; but the after-care of the patient and the anxiety of the operator would be enough to deter most of us from doing house-to-house operating. We must admit that every now and then we are called into the country to do a desperate operation, it being perhaps the last chance to save a patient's life. We operate and save the life of the patient, and the operation may have been done thirty or forty miles away from a hospital. Take the cases of secondary hemorrhage that we had under discussion this morning—every one of you have had them—and you will encounter these cases again in spite of all that has been said. After intra abdominal operations a certain percentage of cases in which secondary hemorrhage takes place will die if twenty miles away from you, but a number of them can be saved if under your supervision. If you reopen the abdomen in some of these cases you will save them. House-to-house operating should be limited to emergency and desperate cases—cases that appeal to humanity for relief. I wish to say that the mortality from house-to-house operating will be greater in the hands of Dr. Ricketts than it would be following operations performed in his own hospital. He cannot deny that. He knows that it is true. I know his work too well, and it is useless for him to entertain the idea that he can get as good results from house-to-house operating as he can in his own hospital. If he had qualified his text and said in emergency cases, I would agree with him in many of the assertions he has made.

Dr. John M. Duff, of Pittsburg, Pa.—The gentlemen who have preceded me have said a good deal of what I was going to say, and there is really no necessity for recapitulation. I am sorry, much as I respect Dr. Ricketts professionally and as a friend, that he read his paper. I am afraid that we sometimes forget the responsible position we occupy when we come to the meetings of this Association. While the

most of us are modest, we must take into consideration the position of prominence we occupy in the medical profession of America. This Association is no longer in the cradle; it has advanced to full manhood, and its dictum will go out all over this country and exert an influence not only upon the profession, but upon the laity as well, and I will be sorry indeed to have the dictum go from here that fully as good work and as low a mortality can be obtained from house-to-house operating as from operations performed in a well-equipped hospital, because the lazy and ignorant members of our profession would have an excuse for not doing their full duty in the line of antisepsis and asepsis.

It has been my lot during the past year to have had under my care a patient for whom I have done considerable surgical work, and on account of her friendship for a private nurse of Dr. Ricketts, that nurse was sent for from Cincinnati to come to Pittsburg, and as far as asepsis and antisepsis were concerned every precaution was taken by this nurse. She was taught by Dr. Ricketts, and to have him come here and give us the sentiments he has today, I cannot understand it. It is true that we have to operate in private houses occasionally, and I presume we always will have to do so, and under the circumstances we must do the best we can. But those of you who have had much experience in general hospitals know very well that the patients who are visited and meddled with by their friends do not get along as well as those who are kept privately. The day before I left Pittsburg I had a conversation with the Superintendent of the West Penn Hospital on this subject, and I told him that I could generally tell when visiting days were held at the hospital because of the exacerbation in temperatures of patients. I believe that is the experience of all of us. Last Sunday I went to see a little boy upon whom I had operated for appendicitis. His temperature was considerably elevated. I had been in the hospital in the morning and went back in the evening to see another patient. I visited the little boy and found his temperature elevated. I asked what the trouble was and whether anybody had visited the little fellow, and the reply was that his father, mother, and sisters were with him for two hours talking to him. The Boards of Directors of hospitals will admit the relatives and friends of patients, and if you attempt to interfere too much there will perhaps be somebody ready to take your place. If an association such as this would stand by men who are working for a reform in this direction and exert an influence on the people who have charge of our hospitals, public sentiment would bring them to demand that which is right in this direction.

There are a good many other reasons why hospitals are better than houses for surgical work. Let us take maternity hospitals. I have had charge of the maternity wards of three different hospitals in the past few years, and I have charge of one hospital in which there is nothing but maternity cases. In the hospital in which I have full control we have not had during the last four years a single case of sepsis; we have not had a single case of mastitis; we have not had a single death from any cause, and I believe it is due to the fact that we carry out antisepsis and asepsis in a rigid manner. When I say antisepsis, I do not mean douching with antiseptics, but the physicians and nurses are compelled to be aseptic; and those of you who do not believe in antiseptics, so far as nurses and physicians in attendance are concerned, should do as Dr. Macdonald has done, have bacteriologic tests made, and you will soon find out where the trouble lies. We do not have the same percentage of recoveries in obstetric work in general practice as we do in hospitals at the present time. It used to be quite the contrary. A woman was afraid to go into a hospital to be confined. Now it is a place of safety, because antisepsis and asepsis are carried out so strictly.

Dr. Robert T. Morris, of New York.—There is one important point in connection with this paper and discussion that has not been brought out. I can train three assistants to get my death-rate down to such small figures that one would think there was nothing seriously the matter with my patients before the operation; but I cannot train six, twelve, or twenty-five physicians, who are in charge of so many other kinds of cases at one time, to give me statistics that are creditable for a report. After all, this matter has to be settled by statistics. The only statistics which I recall to mind at this time are that I have lost eleven appendicitis patients, two of whom died in hospitals and nine in house-to-house operations. At the same time the operations done in hospitals were certainly more than five times as many as those done from house to house. I am speaking now largely from memory, without any statistics before me, excepting that I remember that the difference in the number of deaths was two and nine.

Dr. James F. Baldwin, of Columbus, Ohio.—I must differ from Dr. Ricketts as to the statistics of Dr. Dunlap. His mortality was not 15 per cent., nor do I think it could be represented by three times 15 per cent. I had a talk with Dr. Dunlap some years before his death, and learned from him that he kept no statistics. I have long since learned that the general practitioner who keeps no statistics has a large number of obstetrical cases and a very small death-rate. Dr. Dunlap told me that he thought he had done about four hundred laparatomies,

and that his mortality was about 25 per cent. Dr. J. W. Hamilton, of Columbus, was fully equal to Dr. Dunlap as an operator, and they pursued practically the same methods. Before his death he reported all his abdominal sections, which numbered thirty-six, with nineteen deaths. He told me more than once that he had gone over practically the same field to a large extent that Dunlap had, and had made inquiries among physicians concerning the cases upon which Dr. Dunlap had operated, and he was confident that Dunlap's mortality was over 50 per cent. For the past ten years I have been myself operating in the territory formerly covered by Dr. Dunlap, and am constantly running across reports of his work. I am confident his mortality was over 50 per cent. I see Dr. Reamy here, who is also familiar with Dr. Dunlap's work, and I note that he nods his head approvingly.

Dr. Ricketts (closing the discussion).—If there is anything I love it is a fight, and I certainly have had it here. I was prompted to read this paper for the reason that in looking over my cases the most difficult operations I have performed were in done the homes of patients. Here is a woman who has carried an ovarian tumor for seventeen years, and during the last year of her life she could not get out of bed on account of its presence. I go and operate on that woman, and with clean hands I remove a large tumor containing nine gallons of fluid, and obtain union by first intention. I do abdominal and vaginal hysterectomies, amputate breasts, do appendectomies without pus. Do you pretend to tell me that those of us doing work in this manner have no claims? Gentlemen, I protest. I believe I know that I am right in what I have said. Dr. Carstens remarked that he did not know whether I was sincere or not in what I said. I want to say to him that I believe what I said from the top of my head to the bottom of my feet. I wish you to remember that two-thirds of all the surgery in this country is done in the homes of patients, and in view of this fact I simply ask you to pause and reflect a little. I have made some investigations in regard to this matter of house-to-house operating. I have talked with some gentlemen who are honest in regard to the results obtained from house-to-house operating, and I know whereof I I speak. If emergency cases are operated on at their homes the non-emergency ones can be. There can be no doubt as to this.

As to operations for appendicitis, I do not care what you make use of in the way of antiseptics. I do not care what the color of the water may be as long as I can get results along the lines of simplicity. I really did not expect any of you to come to my rescue this afternoon. You have handled my subject without gloves, and I have a

right to return the compliment. I want to say that we cannot do away with soap and water in surgery, and I desire to remind you again that chemical perfumes of an antiseptic nature cannot take the place of soap and water judiciously applied. The fuss and feathers of surgery must go. So far as taking care of these patients is concerned, they can be cared for as well in their own homes by house-to-house operating as in hospitals. House-to-house operating means house-to-house isolation, and you cannot convince me that there is not more risk of infection in hospitals. The only case of erysipelas I ever had after an operation was in my own hospital. I never had such a case in house-to-house operating. Simple surgical cleanliness is right, and I want to say that ten years from now you will find the pendulum, which has swung to the side of antiseptics and has been carried far out, will swing back to the center of common-sense and cleanliness.

THE TEMPERATURE IMMEDIATELY FOLLOWING ABDOMINAL OPERATIONS.

By LEWIS S. McMURTRY, M.D.,
LOUISVILLE.

Fever is the most constant accompaniment of inflammation, and is the first effect of the inflammatory process upon the general organism to be manifested. While pathologists may differ as to the mechanism of this morbid process, all practically concur that pyrexia is due to the presence in the blood of substances capable of causing it. Such substances are afforded by various systemic diseases and infective fevers. The ptomaïnes evolved and absorbed in the course of gastro-intestinal disturbances are common causes of elevated temperature. The chemical products of bacterial activity are the substances most familiar to the surgeon as causes of febrile action. Tissue necrosis, even without associated bacterial invasion, will beget fever; and fever often accompanies subcutaneous injuries, such as simple fractures, wherein infection is impossible except by bacteria circulating in the blood. Experimental study of fever by Kohler and others indicates that fibrin-ferment when set free in extravasated blood and absorbed through surrounding tissues will cause fever. Another substance found in blood-clot and breaking-down tissues, known as nuclein, will have the same effect, it is claimed. A careful study of the phenomena of shock will convince one that certain nervous phenomena may exert a potent influence in causing fever quite independent of any infection.

As practical surgeons, the subject under consideration is of the greatest interest in its relation to wound infection. Before the antiseptic era all operation wounds were infected, so that so-called surgical fever was the constant and inevitable accompaniment of all wounds. When modern methods enabled the surgeon to obtain primary union after operation without inflammation, it was naturally to be expected that no fever would accompany aseptic repair

of surgical wounds. It was observed, however, that an elevation of temperature commonly follows all surgical operations of magnitude, even when a painstaking aseptic technique has been observed and primary union follows. The researches of Billroth, Volkmann, and Bergmann upon this form of post-operative pyrexia are most interesting and important. Billroth attributed this rise of temperature to the absorption of tissues in and contiguous to the wound which had been devitalized and broken down by the traumatism of the operation. He termed it traumatic fever. Bergmann ascribed the pyrexia to the absorption of fibrin-ferment, which I have already mentioned as shown by experiment to cause fever. He suggested the term fermentation fever. Volkmann attributed the condition to the absorption of disorganized tissue in the wound, and proposed the term aseptic fever.

These observations are of great interest to all surgeons, and deserve especial consideration from those constantly doing abdominal and pelvic surgery. The ominous significance of an elevated temperature immediately following abdominal section is appreciated by surgeons generally. The relation of this symptom to post-operative septicemia makes its careful study and elucidation of the utmost moment.

The essential and all-important point in this inquiry is as to whether there is such a post-operative fever as to justify Volkmann's term of aseptic fever. In well-appointed and properly conducted hospitals where primary healing is the rule it is constantly demonstrated that immediately after abdominal section (beginning in the afternoon of the day of operation and lasting from twenty-four to seventy-two hours) there is a rise of temperature ranging from 99.5° to 101° F., even in cases of aseptic healing without suppuration. Clinical observation daily confirms the experimental demonstrations of Welch, that the blood is endowed with positive germicidal power capable of overcoming pyogenic bacteria, so that wounds heal primarily and aseptically despite minor degrees of infection. The efficiency and limitation of this resisting function of the blood depend more upon the quantity of infecting material (the number of germs) than anything else. The difficulty and imperfection of skin disinfection, both as to the field of operation and the surgeon's hands, are so well known that it is apparent that a slight degree of infection obtains in all operation

wounds, and does not lead to suppuration nor necessarily prevent so-called aseptic primary union. Hence, primary union cannot be accepted as absolute proof of perfect freedom from germ infection. So-called aseptic fever in many instances is in reality the result of a minor degree of infection.

The magnitude and extent of this minor degee of infection must of necessity vary within wide limits. It may extend all the way from a so-called aseptic fever to the marked febrile action of undoubted infection. Premature exposure and dressing of operation wounds favor infection; and if we are to avoid such unnecessary exposure we must be able to interpret correctly the rise of temperature. On the other hand, if drainage is to be established and tension relieved, the opening of dressings is a necessity which cannot be disregarded. In this the thermometer is our principal guide.

In determining this important point, all the ordinary systemic diseases which beget fever must be excluded. Among these I would especially mention malarial fevers and typhoid fever. Gastro-intestinal disorders with absorption of ptomaïnes are frequent causes of fever, and this can be readily determined and corrected by a brisk purgative. The characteristic pulse will give aid in detecting a pyogenic fever; the pulse rising with the temperature.

The distinguishing peculiarity of all the absorption fevers, including so-called aseptic fever, is their manifestation immediately after operation. The profound infection which prevailed in pre-antiseptic days rarely manifested itself before the end of the second or beginning of the third day. The temperature of a stitch abscess does not appear until much later. The absorption fever which has been termed aseptic may quickly subside, or it may persist for many days and give much anxiety in a case wherein all other symptoms are favorable. As already stated, the pulse will usually afford valued assurances of safety.

All cases of immediate fever following abdominal operations, however, are not to be attributed to absorption. In a certain proportion of cases the fever is due to the reaction that follows shock. The action of prolonged anesthesia on the kidneys, heart, stomach, and lungs may have much to do with this, when every precaution is taken to prevent shock by the retention of body-heat. Here, again, the rate and quality of the pulse will afford reliable aid in excluding sepsis.

DISCUSSION.

Dr. James F. Baldwin, of Columbus, Ohio.—I have noticed for several years a rise in temperature taking place almost invariably a few hours after, or more frequently within twelve hours following, these sections. I have long ceased to be anxious in these cases if the pulse and the general condition of the patient continue good. I have not had the fear of infection, from the fact that the fever does not follow the course of ordinary infectious fevers, but comes on too soon after the operation for infection to have taken place. I have noticed a point which the essayist does not call attention to, namely, that in those cases in which temperature goes up to 100½°, and seldom beyond that, except in nervous women, there has preceded it, following immediately after the operation, a depression of temperature, due, I suppose, to some form of shock and not to hemorrhage, because this decrease in temperature occurs without regard to hemorrhage. The nurse who has not had much experience sometimes ominously puts her finger on the temperature-chart when I make my visit, showing that it is going up. I glance at the pulse-record of the same chart, and finding it nearly normal am ready to ignore the rise in temperature. I regard it as simply a reactionary fever. There has been a little shock and depression of temperature, and, like the bounding of a rubber ball, the temperature as reaction comes on rises above the normal, and I regard it in these cases as usually of nervous origin. In this connection I recall my first or second vaginal hysterectomy, made a good many years ago. My patient was exceedingly nervous, and so was I. Her temperature before operation went up to 102°, the pulse accordingly. I could find nothing to account for it. Regarding it, therefore, as being purely of nervous origin, the hysterectomy was proceeded with, when the temperature immediately dropped to normal, and she made a beautiful convalescence.

Dr. Thomas B. Eastman, of Indianapolis, Ind.—I have been very much interested in the excellent contribution of Dr. McMurtry. I believe the majority of abdominal surgeons place more importance upon the pulse as an indicator of the condition of their patient than they do upon the thermometer; yet it cannot be denied that in the thermometer we often have a very good instrument to tell us about certain things in the case. As the essayist intimated, the time at which the temperature appears is of considerable importance. For instance, we get a reactionary temperature within the first twenty-four hours, and we usually disregard it. Let us put our patient to bed;

she gets along nicely; the temperature ranges from 99° to 99.5° during the first twenty-four or forty-eight hours. On the evening of the third day we get an insidious rise of temperature; it runs to 101°, or perhaps not over 100.5°. We observe a corresponding increase in pulse-rate. The temperature runs up to 103.4°, and for a while the pulse has a good volume. It does well, although rapid. The temperature may reach 104°; we have a sudden drop in arterial pressure; the patient becomes cold; antipyretics or cold baths have no effect. The higher the temperature goes the colder the patient gets, and the next thing we know we have a case of sepsis, and are going to have a death in spite of all we can do. Then, again, if we have a high temperature without much effect upon the pulse, along toward the sixth or seventh day, we simply open up the wound and expect to find a stitch abscess. There is one thing I have observed, namely, that as long as the patient has a good, warm skin, the skin warm in proportion to the temperature, we need not regard it as of great importance. But it is the patient who does nicely and ideally, so to speak, for the first three days and on the evening of the third day has a sudden rise of temperature with a corresponding increased pulse-rate that worries me, and I expect a death shortly.

Dr. J. Henry Carstens, of Detroit, Mich.—I heartily agree with the essayist in the position he has taken. If we have a case of suppuration and operate on it, what do we do? We open up a lot of lymph channels, create raw surfaces, and in spite of the greatest care there will be a transudation of some of the septic material, which will come in contact with the raw surfaces and the open lymph channels, and the result is that as soon as we get through with our operation there is infection from the absorption of a great amount of material, and no matter how much we flush we cannot get rid of it. The temperature rises. Nature very kindly, by phagocytosis, takes care of the septic material, and in the course of twenty-four hours it is eliminated, and the patient is all right. That is the way I have explained this early rise of temperature. Undoubtedly, the temperature will rise or fall without any particular reference to bacterial infection, and there is not the slightest doubt at all that without bacterial infection we can have an elevation or lowering of temperature, and if we have it at other times, we can likewise have it after operations, so that absolutely we cannot say that it is bacterial. I think, however, as a general rule, after an operation we had better try and be on the safe side, and consider it bacterial.

Dr. John M. Duff, of Pittsburg, Pa.—In connection with the paper of Dr. McMurtry, I desire to present two temperature-charts,

showing that the temperature arose above the point which our ordinary thermometers record. I have forgotten the exact temperature, but I believe in one it was 117° F., in the other 120° F.[1] In one case I did an operation, simply a curettement, and was very much alarmed to find the temperature had risen to 107°. For a little while it was 110°. My patient was extremely nervous and became delirious. A neurologist said that I had a case of brain fever. I went back to see the patient an hour or two after the operation, and found the temperature normal. In a few hours again it had arisen to 110°. I will send Dr. McMurtry these charts, which I think will be of interest to him in this connection. One of the patients had several hysterical attacks during which the temperature was 108°.

Dr. W. E. B. Davis, of Birmingham, Alabama.—Unquestionably there can be elevation of temperature without infection. The experience of surgeons with injuries of magnitude, the skin being intact, where there has been depression and a corresponding elevation of temperature, should demonstrate this. Dr. McMurtry emphasized the very important fact that much depends upon the dose of infection. There are a great many cases of infection without the formation of pus; the wound heals by first intention, but there is infection sufficient to cause elevation of temperature. Then, too, we have patients who bleed freely, and these cases with very little infection will have considerable elevation of temperature after an operation.

After an abdominal operation of severity there is diminished bile production for a time, the urine is reduced in quantity, and injury to the abdominal sympathetic ganglia produces paralysis of the intestines. Auto-infection is a frequent source of fever, as is demonstrated by the rapid reduction in temperature after free purgation. It is not a beginning of peritonitis that causes elevation of temperature in a very large proportion of cases.

Dr. Hugo O. Pantzer, of Indianapolis, Ind.—My observations and experience have led me to favor the position taken by the writer of the paper. Any rise in temperature should ordinarily be construed as being due to infection. I cannot take the extreme stand-point that nervous influences cannot bring about elevation of temperature. They undoubtedly do at times, but I am rather chary to say so in the individual case. I am satisfied that a rise in temperature, early after an operation, is often owing to a nasal septicemia. The presence of nasal catarrh accompanied with septic conditions is of frequent occurrence, more than generally accepted. A rise of temperature, owing to the

[1] Dr. McCann informs me the temperature in the one case was 123½° F. Dr. Hersman informs me the other was 117° F.

nasal conditions, is favored by the change the patient has recently undergone. From an alternating, upright and recumbent position we have the change to protracted recumbency, which will favor occlusion of the nose, with retention of nasal secretions, conditions which favor bacterial propagation, the formation of toxins, and the ultimate development of septicemia.

There is one point more pertaining to temperature to which I desire to refer. The old surgeons did not determine the presence of fever by the temperature of the mouth, as is the routine practice of to-day. They placed their hand over the inflamed area. The truest registry of the fever in abdominal and pelvic affections is obtained either in the rectum or in the vagina. Taken in these places our records will oftener show a rise of temperature after operation, or a higher degree of temperature than where this is taken by the mouth or axilla.

I take this occasion to express my appreciation of the honor conferred upon me in being made a Fellow of this Association. I wish to say that in the thesis which I have handed to the Secretary this subject is treated, and I may desist from further remarks on this subject here.

Dr. Walter B. Dorsett, of St. Louis, Mo.—The matter of ptomaïne poisoning has been dealt with to some extent, but nothing has been said about the kidneys. I did an operation on last Wednesday, a vaginal hysterectomy, for carcinoma, and I know of no operation that I have done in which I felt more anxiety as to the condition of the patient afterward. About twenty-four hours after the operation the total amount of urine excreted by the kidneys was two ounces, and the temperature went up to 103°. Chloroform was given as an anesthetic. The vomiting was so continuous and prolonged that it was impossible to give her any liquids whatever by the stomach, and on making a chemical examination of the urine I found it to be almost half albumin. I resorted to transfusion with normal salt solution, as well as enemata of normal salt solution, carried high up into the rectum, and was rewarded in the course of twelve or fourteen hours by a reduction in temperature to 99½°, and it has not risen above that point since. I think we ought to consider always the anesthetic that is used, and next the condition of the kidneys. I believe that is the reason why we observe abnormal temperature after operations. After I used the normal salt solution the excretion of urine was markedly increased. An hour after the transfusion we got four ounces of urine, the next time six ounces, the next ten, at intervals of three hours apart, so that I believe the temperature is often influenced by the ability of the kidneys to functionate, and is not due to any septic material that may enter the blood.

DR. MCMURTRY (closing the discussion).—I will detain the Fellows but a moment. There is no difference of opinion whatever as to the influence of nervous conditions in producing high temperatures. It is a very well-known fact, clinically observed frequently, that either from physiologic shock or psychic shock, the temperature runs up to a very high point in surgical cases. The same holds true with regard to the influence of systemic conditions, such as gastro-intestinal disturbances, as mentioned by Dr. Davis, impaired action on the part of the kidneys, and of other excretory organs or functions, as mentioned by the last speaker. Those are all well-known conditions. What I desired especially to call the attention of the Fellows to was the uncertainty of the classic explanations now given as to those elevations of temperature that occur immediately following surgical operations. They are practically attributed to a fibrin-ferment, to the presence of nuclein, and to the absorption of disorganized tissues as a result of the operation. This, it seems to me, is very far from being proven or explaining the condition, and the deductions that are drawn from our present ideas about elevation of temperature are not of very great value. Let us take the illustration that Dr. Eastman gave, as it is thoroughly practical, and that is, whenever you have on the end of the second or beginning of the third day after an operation a high temperature, with a failure of the capillary circulation, as is evidenced by placing your hand on the skin, you have a dissemination of septic infection which is certainly the precursor of a fatal issue. These are very valuable points in regard to temperature. As Dr. Eastman stated, a little later in the case you find a certain elevation of temperature; you have only to remove the dressings, and you will find the explanation and cause for it before you.

The object of my paper is to study these subjects and present them in a manner that our observations can be more thoroughly utilized in a practical way. The systemic conditions that will beget a rise of temperature are to be considered. Reactionary fever; the fever that accompanies and follows shock; the fever that comes from nervous influences of various kinds are to be considered. We will find that all manner of degrees of infection will obtain in spite of the ordinary precautions that are taken. Imperfection in surgical technique will in a great many cases give us a rise in temperature which we know is ascribed to extraneous influences.

INTESTINAL ADHESIONS IN SUPPURATIVE PELVIC DISEASE: THEIR SIGNIFICANCE AFTER VAGINAL HYSTERO-SALPINGO-OÖPHORECTOMY.

With Report of a Case of Ileus.

By F. BLUME, M.D.,
PITTSBURG.

There is perhaps no subject in the whole range of surgery which within the past few years has been so frequently discussed in societies and journals as the management of suppurative pelvic disease. Yet, in spite of all these discussions, the question how to deal with the various forms of pelvic suppuration is far from being settled, and even the prospects of establishing more precise indications for the treatment of these conditions in the near future are by no means encouraging.

New methods of treatment, like new drugs or other innovations, if introduced by men of great reputation, seldom fail to arouse enthusiasm long before their value is really established. If they appear to be of great importance, the enthusiasm grows and soon spreads, contagion-like, irresistibly sweeping everything before it. Such was pre-eminently the case with vaginal hystero-salpingo-oöphorectomy after its recommendation by Péan for a certain class of cases of pelvic suppuration. Though vigorously attacked by the exponents of the suprapubic operation, it has well stood the test of time, and, if limited to those serious bilateral suppurative lesions which preclude conservative methods, it is looked upon to-day by many surgeons as a life-saving operation, a benefaction to womankind.

Anyone who, without prejudice, studies the literature of this subject must admit that the advocates of the radical vaginal procedure have ably defended their position and that the weight of

argument is overwhelmingly on their side. It cannot be denied, however, that some of the writers, carried away by their enthusiasm, have overdrawn the picture when they speak of rapid recovery, patient out of bed in a week or ten days, no complications and sequelæ, mortality practically *nil*.

On the other hand, one of the most perplexing things is the lack of argument in the attacks of the partisans of the abdominal operation. Painting in too glowing colors the picture so well known to all of us, they describe the abdominal route as the ideal method; recovery uneventful; patient, though able to be dismissed sooner, left the hospital in excellent condition at the end of three weeks; mortality slightly higher than after the vaginal operation, but no complications and sequelæ; ventral hernia unknown or rarely seen in their own cases.

Those who from personal experience are familiar with the advantages and disadvantages of both procedures cannot be misled by such enthusiastic writers. They must, however, deeply regret the one-sided manner in which this subject has been so frequently debated. The usual terms, pyosalpinx, suppurative pelvic disease, and the like, comprise a variety of lesions which differ essentially in their etiology, pathology, and symptomatology. From this point of view the various methods of treatment and their results must be studied. The history of the cases, whether acute or chronic, the condition of the patient, and the possibility of complications must also be taken into account if just and satisfactory conclusions shall be reached. Simply to object to the vaginal operation because one believes that the removal of the uterus takes away the keystone of the pelvic arch, that it spoils the pleasurable sensation experienced during cohabitation, or because of some other equally unfounded and unimportant reason, does not help to throw light upon the subject. Such objections do not account for much in the face of the excellent results which have been and are daily obtained with this life-saving operation. They stand on a par with the reasoning of that class of vaginal enthusiasts who, restricting the abdominal route almost exclusively to large tumor cases, are ready to operate through the vagina for any morbid condition that may befall the pelvic organs. Lawson Tait,[1] criticising the assertion that after the removal of the uterus the sexual appetite is lost and

[1] Buffalo Medical Journal, May, 1899, p. 731.

the sexual relations are disturbed, well hits the mark when stating, in his characteristic manner, that he does not regard the "sexual appetite argument" as worthy of any but the brothel-keeper. If the profession at large has not yet arrived at this conclusion, it is rapidly approximating thereto.

Though it is not intended to review the objections raised against vaginal hystero-salpingo-oöphorectomy, I cannot conclude my introductory remarks without briefly commenting on a remarkable paper recently published by Dr. J. M. Baldy.[1] This paper is of interest because its author attempts to establish a certain standard of death-rate for the suprapubic operation and to thus abolish the vaginal procedure. Based upon a series of seventy-three cases of inflammatory pelvic lesions with one death, a brilliant result which any operator may justly feel proud of, Dr. Baldy arrives at the conclusion that a 5 per cent. mortality should be considered the standard rate. All cases in the tables, as the doctor states, are arbitrarily divided into salpingitis and pyosalpinx, to indicate the absence or presence of pus. Analyzing them we find that 25 are recorded as double pyosalpinx, 2 as unilateral pyosalpinx, 1 as puerperal phlebitis and lymphangitis, and the remaining 45 as double salpingitis. Among the 25 patients with double pyosalpinx, the one death occurred, a mortality-rate of 4 per cent. As the indications for vaginal hystero-salpingo-oöphorectomy should be limited to serious cases of bilateral suppuration, it is this group of cases only which interests us here. It is evident at first sight that a series of 25 cases—or, adding the case of puerperal phlebitis, 26 cases—though it may show what a skilful surgeon can accomplish, is far too small to be of value in the decision of such an important and complicated question. The fact that, with few exceptions, the mortality of the abdominal operation for these grave lesions is still exceedingly high, varying between 17 and 25 per cent., is verified by the records published in our journals.

In a paper which I had the honor to present to you at our Pittsburg meeting I reported the complications with which I had met in 3 cases in a series of 42 vaginal hystero-salpingo-oöphorectomies. In one instance only the complications were unavoidable—a case of ileus due to intestinal adhesions antedating the operation. This pathological condition of the bowel complicating the pelvic lesions

[1] American Journal of Obstetrics, May, 1899.

has led to objections to the vaginal operation. It is especially from this point of view that a distinguished Fellow of our Association, Dr. Joseph Price, has discussed the subject and advised against the vaginal route whenever the opportunity presented itself. Some of his remarks made in the discussion of my paper have been exceedingly interesting to me, especially the following sentence: "Given a case with vicious disease of the tubes and ovaries without complications above, or if I can dismiss complications of every character above the uterus and appendages, then I should say the vaginal operation is the operation above all others." These words, coming from an abdominal surgeon with an enormous experience, are worthy of earnest consideration. While the superficial reader, perhaps, may regard them as a condemnation of the vaginal procedure, to me it sounds quite well when one of the most successful abdominal surgeons declares that in serious bilateral lesions the vaginal operation is the operation above all others if suprapelvic complications can be dismissed.

I have always held the opinion that the value of an operation does not depend upon the immediate result alone. Any operative procedure which is not curative in the vast majority of the cases does not deserve recommendation, no matter how brilliant the immediate results may be. The very fact that the radical vaginal operation is curative, and that the serious post-operative sequelæ which often follow the abdominal procedure are absent, has led me to adopt the vaginal route. It is from this point of view that I discussed the complications which I had observed in my vaginal work for pelvic suppuration in the paper read before you last year, of which this brief article is the continuation. From personal experience I am forced to admit that in some instances grave disturbances may result from the coexisting suprapelvic lesions, and that of all the objections the complication theory is the most important, deserving further investigation.

We all remember too well the complications with which we have met in our abdominal operations for suppurative pelvic disease. The pathological conditions in this group of cases are by no means always confined to the uterus, the appendages, the bladder, the rectum, and the sigmoid, where they possibly could be completely relieved by the vaginal operation. In many instances the inflammatory process extends to the abdominal cavity, involving a large

part of the omentum and many inches of that portion of the small intestines overlying the diseased pelvic structures. Adhesions are found to have formed between various loops of intestines, between bowel and pelvic wall, between bowel and omentum, or omentum and bladder, as the case may be, according to the extent and severity of the inflammatory process. These intestinal and omental lesions, which cannot be dealt with by the vaginal route, are frequently observed in secondary abdominal sections. They are of common occurrence after the ablation of the appendages by the suprapubic operation, and in many instances accompanied by an inflammatory process about the pedicles, resulting in the formation of exudates.

It is well known that general adhesions between the coils of small intestines do not interfere with the peristaltic action, and the advice of writers with great experience, not to break them up when operating suprapubically, deserves commendation. They are no more an obstacle to the vaginal route than those which bind bowel and omentum to the diseased pelvic organs and which are easily separated. The formation of peritoneal bands, however, or of adhesions between the ileum and the pelvic floor, it must be admitted, cannot be regarded as harmless complications. The statement that under these circumstances the descent of the bowels following the vaginal extirpation of the uterus and appendages favors intestinal obstruction, is as true as the assertion that this grave complication is of the rarest occurrence. While I have met with 2 cases of mechanical ileus in 51 vaginal hystero-salpingo-oöphorectomies, other operators, reporting large series of 100 and 200 cases, have never, or but rarely, seen this complication. Upon inquiry I learn that of our Pittsburg gynecologists I am the only one who observed ileus following the vaginal operation for this class of disease. The records published within the past few years, representing many hundred cases, demonstrate beyond dispute that we need not fear the suprapelvic lesions. From the evidence which they present we are justified in saying that the vaginal operation is the operation above all others in those grave cases of pelvic suppuration in which both appendages must be removed.

My first case of intestinal obstruction occurred in a series of 42 vaginal hysterectomies and was reported last year. Since then I have done 9 vaginal hystero-salpingo-oöphorectomies for pelvic

suppuration and have had another case of ileus, the history of which I now submit to your kind consideration.

Mrs. E. M., aged twenty-one years; married; menstruation at eleven, normal; one child, nine months old; labor and puerperium normal. Patient was in good health until about two months ago, when she was suddenly seized with severe cramp-like pains in the lower part of the abdomen, followed by fever lasting four weeks. Uterus large, retroverted, adherent, and surrounded by hard masses of a bony consistence which filled the pelvis posteriorly and on both sides. Bacteriologic examination showed streptococci. Gonococci could not be found. Pelvic peritonitis due to streptococcus infection. The cause of the infection could not be determined. It looked like a case of abortion, but the patient positively denied having been pregnant.

Vaginal hystero-salpingo-oöphorectomy, May 31, 1899. The operation was difficult, the uterus being immovable and so friable that the forceps tore out at the least effort at traction. Scattered throughout the indurated tissue were numerous small pus-cavities. Both tubes were large, but contained only a few drachms of pus. Their walls were friable and of enormous size, especially at the fimbriated extremities. The amount of pus removed was small, scarcely more than one ounce. Bacteriologic examination showed the streptococcus pyogenes.

The patient reacted well under the free use of stimulants, and made a good recovery. From June 4th she improved rapidly. Her temperature was normal; the pulse remained accelerated until June 12th, varying between 90 and 100. On June 14th, the fifteenth day after the operation, she complained of severe abdominal pain, and from this day on the symptoms of intestinal obstruction gradually developed. The pulse-rate increased to 100. Nausea and vomiting. The abdomen became tympanitic; increased peristalsis was visible through the thin abdominal walls. The bowels, which daily had promptly responded to mild cathartics, became more and more constipated, until, after June 17th, all attempts to obtain a movement resulted in failure. Thirty-six hours later, the twentieth day after the operation, I opened the abdomen. The general condition of the patient was still good; pulse 120, temperature 98°, skin slightly cyanotic.

Celiotomy, June 19, 1899. Omentum adherent to abdominal

wall was separated. At once two distended coils of the ileum appeared in the abdominal incision, one of them being rotated more than half-way about its longitudinal axis—partial volvulus—and kept in this position by several firm bands. As soon as these bands were cut between ligatures the collapsed portion of the bowel below the constriction became distended. The omentum which was adherent to the bladder and underlying bowel was next released, ligated, and a large portion of it excised. It could now be seen that numerous coils of the ileum were involved, being adherent among themselves and to the pelvic floor. After all the adhesions were broken up the loops of the intestines were drawn out, stitched at different points, and replaced. Abdomen closed.

The patient left the table with a pulse of 140, received a pint of normal salt solution under each breast, and rallied quickly. She made a most excellent recovery. Gas expelled by rectum fifteen hours after operation; bowels moved on third day. Abdominal sutures removed on the fourteenth day; primary union. Patient discharged in excellent condition July 11th, the twenty-third day after the second operation.

The question suggests itself: Was the radical vaginal operation indicated in this case? Palliative treatment was faithfully tried for over a month after the acute attack had subsided. It utterly failed to improve the patient. Vaginal incision and drainage was seriously considered, not as a curative but as a temporary operation. This simple procedure, so valuable in large accumulations of pus, is especially indicated in acute suppurative conditions if the presence of purulent collections can be made out. Any attempt, however, to drain such thick and indurated tissue with its multiple centers of suppuration, as in the case under discussion, not to speak of the tubes with walls more than half an inch in thickness and but a small amount of pus, must result in failure. I know from personal experience that this condition, so characteristic of streptococcus infection, cannot be even temporarily relieved by vaginal incision, and that other operators have come to the same conclusion.

Abdominal section was out of the question in this acute case of streptococcus infection, the danger of sepsis being too great. The contamination of the peritoneal cavity by virulent streptococci, so difficult to avoid in the abdominal operation, is quickly followed

by peritonitis, and in many instances by death. Therefore, it is a wise plan to attempt to relieve these patients by palliative treatment and to postpone the radical operation until the bacteria have lost their virulence. If palliative means fail, or are deemed inefficient, as in this case, if the social condition of the patient will not permit her to remain in bed an invalid for half a year or longer until the bacteria have ceased to live, then vaginal hysterectomy should be the operation of election as the least dangerous procedure.

The time has passed when a surgeon could say, without jeopardizing his reputation, that he operates exclusively by the abdominal or the vaginal route. Since the introduction of vaginal hysterectomy and the revival of vaginal incision and drainage, our views regarding the treatment of suppurative pelvic diseases have undergone remarkable modifications. We have learned to appreciate the value of the differential diagnosis between the various forms and stages of the disease. We realize that there is no longer but one method of surgical treatment, and that in order to do justice to the patient we must select that method best adapted to the case.

I have seen many women who, after abdominal section for pelvic suppuration performed in this country and abroad, were invalids, and I have done secondary operations upon a number of them. But neither have I been called upon to treat a woman upon whom vaginal hysterectomy was performed by another surgeon, nor have, to my knowledge, any of my patients required or sought treatment at the hands of other operators. My experience is in accordance with the observations of other writers, and justifies the assertion that the results of vaginal hystero-salpingo-oöphorectomy in suppurative pelvic disease are excellent, notwithstanding the suprapelvic complications, and that there is no class of cases which, as a whole, gives more satisfaction.

DISCUSSION.

Dr. J. Henry Carstens, of Detroit, Mich.—Mr. President: I have gradually come around to the belief, in connection with septic tubal diseases, that the vaginal operation is, as a rule, the easiest and simplest. You will remember that after a great deal of hard work at the last

meeting of the Association, in asking Dr. Price questions, that he made the remarks quoted by Dr. Blume in his paper—namely, that if he was convinced that there was no trouble in the abdomen above the pelvis, that by all means he would resort to the vaginal route, and that is the position I have been taking, provided I know that there is no complication. We are never absolutely sure that there is none; but from the history of the case, from our examination, etc., we will in nine times out of ten be right, and know that there is no complication above, that the adhesions are below, and that we can remove the uterus and the tubes by the vagina. It is remarkable how easily we can enucleate the uterus and tubes from below and loosen the adhesions, while by operating from above we have great difficulty in doing it. For that reason, I have gradually come around to the plan of practising vaginal hysterectomy in double pus-tubes. Whenever I think there is a complication in the abdomen, and especially when I get a history which seems to point to involvement of the appendix, *a by no means uncommon affair*, then I take no chances, but open the abdomen in order to remove the appendix and pus-tubes. I have found by experience what Dr. Blume mentioned, that in those cases of pus-tubes which require to be opened by the abdomen they are difficult to enucleate and are complicated with extensive adhesions, and perhaps involvement of the appendix, etc. The mortality attending these cases is much greater. My experience coincides with that of Dr. Blume, namely, that in removing double pus-tubes by the vaginal route the mortality is almost nothing, while in opening the abdomen for the same purpose it is very large. Of twenty-one cases I operated on last year four died.

There was one point Dr. Blume referred to which I desire to emphasize, and that is the peculiar tendency of some men to make assertions that we have no business to have a mortality of so-and-so in operating for pus-tubes by abdominal section, etc., say a mortality of 3 or 4 or 5 per cent. I deprecate the making of any such remarks. No one has a right to make such a statement. All of us, every once in a while, have a very long series of cases, say twenty or fifty, with a very low mortality attending our operations, and on this account we are liable to get swelled heads, and we think we can do wonderful things. By and by, when we have different cases to operate upon, and our results are attended with a greater mortality, our swelled heads become much smaller. We get meek and humble. That has been my experience. When a man with a very limited experience in abdominal surgery, who has perhaps only a few cases in the course of a year, talks to me in that kind of a way, and says that I should not have a larger

mortality than 1 or 2 per cent., I have my opinion of him, and I am inclined to tell him that when he gets older and has had more experience he will not have such a big head.

Dr. Edward J. Ill, of Newark, N. J.—I agree in the main with what Dr. Blume has said with reference to operating on double pus-tubes, etc. There is a class of cases which we should always operate upon by the abdominal route, and it is true that in operating on some of them by the vaginal route we do incomplete surgery, particularly where we have to leave some pus-cavities and extensive adhesions. When we can define the exact limitation of these pus-tubes and adhesions, we can do very well by the vaginal route. I am very glad indeed to hear Dr. Blume say that no conscientious surgeon will adopt one route to the exclusion of the other. Such a course is certainly a mistake. No one route is suitable for every case, and sometimes it takes very close discrimination to differentiate.

Dr. W. E. B. Davis, of Birmingham, Ala.—This question hinges largely upon whether the uterus shall be preserved or not. The French surgeons claim that it is best to remove the uterus, and if we accept this as the best practice, in cases where both appendages are to be removed, then the vaginal operation should have a much wider field than it has held in this country. If, however, the uterus is to be preserved, then the abdominal route must be the method of operation and the vaginal route the exception. It is in my judgment a mistake to accept the teachings of Kelly and others that the uterus should be sacrificed in all cases where both appendages are removed. In cases of puerperal abscess where the abscess is peritubal and periovarian the vaginal route affords many advantages for their proper treatment. The abdominal route, with the Trendelenburg position, will enable us to reach the other cases better than by vaginal section.

Dr. Willis G. Macdonald, of Albany, N. Y.—It seems to me that many of us have made similar speeches in discussing this same subject on previous occasions. Both sides have set up a long series of wooden men to knock down. There are a number of surgeons who are coming to the belief that not all the good surgery that has ever been done upon pus-tubes and collections of pus, has been accomplished through the abdominal incision. A considerable number of surgeons, who are doing a fair amount of work, are becoming convinced more and more that they can do good surgery and effect cures through the vagina. The simple matter of statistics of recoveries or deaths, or the number of patients living at the time of their discharge from the hospital, does not cut any very extensive figure in this matter. There are a number of cases in which the ovaries and tubes have been removed

for gonorrheal disease that are far from being cured, if we accept the testimony of their companions; that there yet persists an infectious discharge from the uterus, which is somewhat embarrassing at times, and this discharge sends the patients back to other surgeons for treatment. The use of the curette is not always effectual in removing this disease. Those are facts which are observed from time to time, and many of these patients are relieved of their difficulties, pains and distress by vaginal hysterectomy. I cannot quite appreciate Dr. Duff's inverse reasoning in the matter, when he says it is a good thing to take the uterus out, while it may be a good thing to allow it to remain. But I am sure the paper which was read represents a series of good work in this particular field. Until two years ago I had never attacked a pus-tube through the vagina. I have not counted the number of cases I have operated upon in this manner, but I am sure it is more than fifty. I know the final result of this number of cases, and while a fair proportion of them with the uterus suffered from little foci of pus, the size of a pea to that of a walnut, such cases as Dr. Blume describes are suitable for the vaginal operation. Again, not all cases of pyosalpinx, nor perhaps half of them, are suitable for the vaginal route. There are other cases in which I am just as earnest that we should operate upon through the abdominal incision. Take an old case in which you have a history of certain symptoms and you know induration has gone on for several months; it is far more difficult, it is far more dangerous, to operate through a vaginal incision in such a case than through an abdominal one. We have entirely different conditions to deal with when the tissues cannot be separated readily, in which we can easily tear into the surrounding organs. In breaking up adhesions one may tear into the bladder, the rectum, or some adherent intestine. Such cases are not suitable for the vaginal route. Those are the cases in which there is danger from pus infecting the general peritoneal cavity. The organisms are dead, and the pus, instead of being virulent, has either become thin, caseated, or the case more nearly approaches hydrosalpinx.

In another class of cases we make a bimanual examination, and the moment the finger touches the cervix it glides by it, or comes directly on a mass in Douglas' cul-de-sac dipping down into the pelvis. You try to lift it by the finger and it does not yield. As you pass the bimanual hand to bring them close together, this material or mass does not lift up at all. In such a case you had better operate by the vagina. On the other hand, there are other masses of a similar nature that you can feel as you put your hand over the symphysis that will lift up slightly. Those are cases of simple ovarian abscess, holding four or

five ounces of pus, and can be taken out through the abdomen and the entire sac enucleated with it.

Dr. T. A. Reamy, of Cincinnati, Ohio (by invitation).—I have no doubt most of you have had the same experience as myself in dealing with the class of cases under discussion. It has been my custom for many years, if the condition of the intestines described above exists, where there is a large amount of pus in the pelvis, not to break up the adhesions, but to protect the general peritoneal cavity by allowing the intestines to remain adherent. Even in cases where there has been extensive involvement of the tubes with pus I have emptied the pelvis from below. I think two ends are served by not breaking up the adhesions. In the first place, you do not have the descent of the intestines and a hernia of the small intestine into the large cavity which has been described. This is prevented by the existence of the adhesions. In the second place, the admission of pus into the general peritoneal cavity is prevented.

As to the evil of leaving the adhesions where I have dealt with pathologic conditions from below, I have been astonished in many of my cases as to the ultimate results, where not only symptoms indicated extensive adhesions prior to operation, but which were detected at the operation, the condition of these patients afterward showing that the adhesions had been disposed of in some way. They have been disposed of by natural processes. Although it seems difficult to explain why it should not be so, yet, so far as I know, I have not had a solitary case where the intestines have descended so as to cause trouble in the future.

With reference to the vaginal operation, there are certain cases suitable for operation by this route, and other cases in which the abdominal method is indicated. In still other cases it becomes necessary to resort to the combined method—namely, both the vaginal and abdominal.

Dr. Blume (closing the discussion).—I shall occupy but little time in my closing remarks. I intended to prove in my paper that intestinal adhesions complicating pelvic suppuration are by no means as dangerous as some surgeons would have us believe. The fact that I have never been called upon to treat a patient upon whom the radical vaginal operation was done by another surgeon, and that, as far as I know, none of my patients have needed or sought further treatment at the hands of other operators, certainly speaks in favor of the operation, and shows that the ultimate results are satisfactory notwithstanding the suprapelvic complications.

There is no longer but one method of surgical treatment in this

class of cases, and the surgeon who still adheres exclusively to the abdominal route makes a serious mistake. The only way to reduce the high mortality rate of the abdominal operation is to differentiate between the various forms and stages of the disease and select that method of treatment best adapted to the case.

SHALL WE ABANDON VENTROFIXATION OF THE UTERUS?

By WALTER BLACKBURN DORSETT, M.D.,
ST. LOUIS.

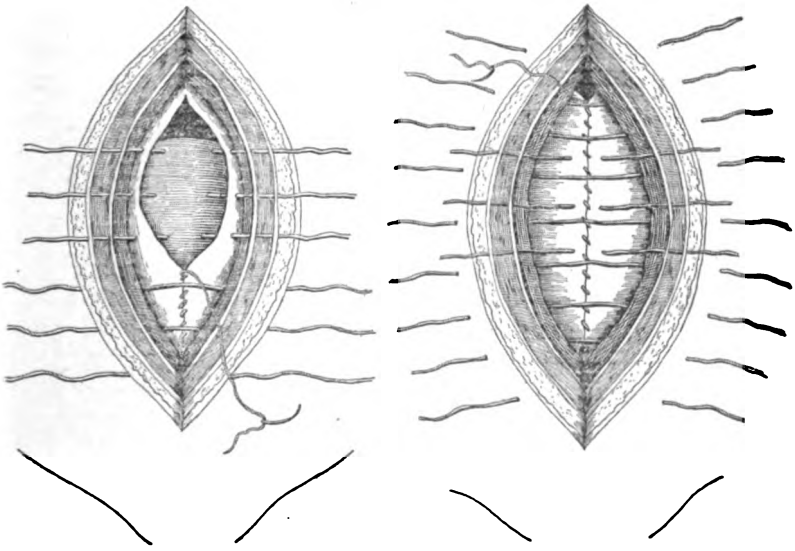
It will be noticed that a question here is asked. This, then, would imply that some doubt exists as to the propriety of the operation of fixing or suspending the uterus to the abdominal wall. The correction of displacements of the uterus has occupied the attention of the profession for many years; and, until gynecic surgery obtained the strong foothold that it occupies to-day, little else, aside from the use of various pessaries and supports, was ever resorted to. It is not my purpose to condemn the use of the pessary in properly selected cases. On the contrary, I am sure it has a place in the treatment of cases of procidentia, versions, and flexions, where previous peri-inflammations have left no adhesions, and where no inflammatory condition of the tubes and ovaries exists.

To properly fit and adjust a pessary in a simple and uncomplicated case of version or flexion is not to consign a woman to the use of a pessary for the remainder of her life. On the contrary, many cases, if treated in time, and properly treated, escape surgical intervention; and the wearing of a properly shaped and adjusted pessary often gives immediate and permanent relief, and, after the correction of the condition for which it was used, can be abandoned. But it must be admitted that there are many cases that haunt the office of the physician which cannot be benefited by the means alluded to and are amenable to nothing short of surgical treatment. To this class of cases the various surgical methods have been applied. The fact that *many* surgical operations have been devised by many operators for the same conditons would imply that no ideal operation now exists, and that heretofore the work done in this direction has been largely experimental. This is accentuated by the fact that by a review of this work it will be found that there are today

good men advocating different methods from what they spoke approvingly of only a few years ago. Total failures and disappointing and unsatisfactory results have followed every operator, in the practice of all the different methods used to correct retrodeviations and descensus of the uterus, since surgical methods have been brought forward.

It is not my purpose in this short paper to discuss the different methods of correction of displacements of the uterus nor the results or accidents that are liable and occasionally follow them; but it is rather for the purpose of drawing attention to one method which,

FIG. 1. FIG. 2.



Showing method of suturing.

on account of the unfortunate results that have now and then followed it, has been to a certain extent abandoned by some operators. It is that of ventrofixation. By ventrofixation, as practised by myself, I do not mean ventrosuspension as originated and practised by Kelly and others. My experience, while limited, has not been followed by the results reported by others who have worked along the same lines. Whether my technique is different and may have some better features I do not know, but the fact remains that the results have all been highly satisfactory in my hands. Up to

four weeks ago I had made thirty-eight ventrofixations, and up to this time have had no failures so far as to keeping the uterus in its proper place. Four have borne children without any of the usual troubles reported in pregnancy or during the delivery of the child, and none have had entanglements of the bowels or omentum. Of these, eight were done to correct different degrees of prolapsus, and the remainder were for retroflexions and retroversions. Fifteen of the latter were complicated with ruptures of the perineum and cervix, which were previously repaired by myself or others before the final operation was done. Sixteen were in married women of the child-bearing age and between the ages of twenty-three and thirty-nine years. Twelve were in cases of marked disease of the

FIG. 3.

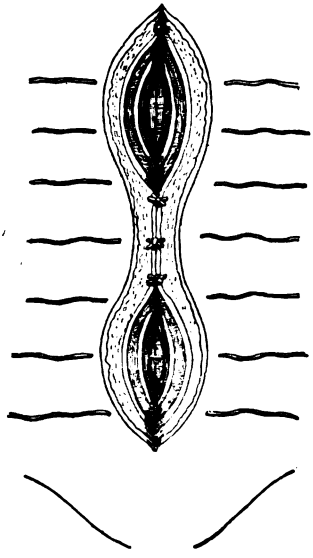
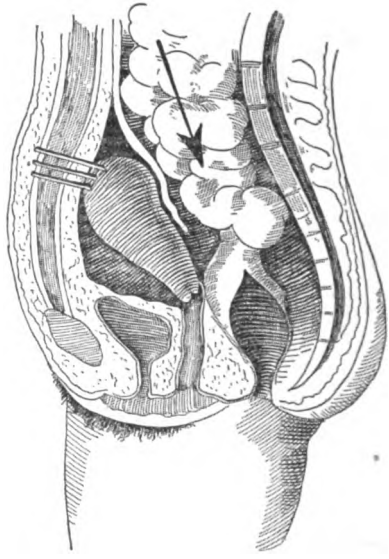


FIG. 4.



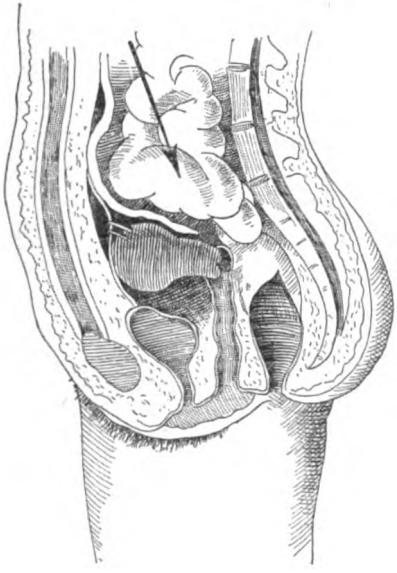
Uterine stitches tied and fundus held against anterior abdominal wall. The arrow shows direction of intra-abdominal pressure.

adnexa; five of double pyosalpinx and abscess of one or both ovaries; and two of disease of one tube and one ovary. Six were in marked neurasthenic women in whom there was a pronounced relaxed condition of the entire muscular system, predisposing the individual toward retrodeviation.

Technique of the Operation. The patient is anesthetized by chlo-

roform and placed in the Trendelenburg position, the abdomen and pubes having previously been cleansed and prepared according to the approved antiseptic methods of the day. An incision, ranging from three and a half to four and a half inches long, according to the thickness of the abdominal wall, is made in the median line, greater care being taken than is usual to follow the linea alba, for reasons to be hereafter stated. As soon as the abdominal cavity is reached the right hand is introduced with the palm looking toward the pubis. Adhesions are now sought for, and if any are found

FIG. 5.



Showing stripping of peritoneum from abdominal wall when sutures are improperly placed. The arrow shows the direction of intra-abdominal pressure.

they are divided by inserting and cutting between ligatures. If the tubes and ovaries are diseased they are resected or removed. The uterus is then lifted up by grasping it at or a little above the fundus, with the thumb on the anterior surface and the fingers on the posterior surface. It is now held forward and upward by a double tenaculum forceps attached at the fundus by an assistant. A double curved needle, armed with an ordinary kangaroo tendon, is now inserted and passed through the sheath of the rectus muscle on the anterior aspect of the rectus, and passed around the inner

border of the rectus, emerging from the posterior layer of the sheath about one-eighth of an inch under the inner border, in order to grasp from three-quarters to an inch of the sheath. It then passes out of this sheath and through the subperitoneal fat and peritoneum, emerging about a quarter of an inch from the incision. By following this course the attempt is made to grasp the sheath of the muscle only and avoid taking in the muscular fibres. The needle is now inserted at a point about an eighth of an inch to the right and below the center of the fundus, on the anterior and not the posterior wall, and emerges at a corresponding point on the left side of the organ, and passes through the same structures as in the opposite side of the incision in the anterior abdominal wall, but in a reverse order. The sutures are now held by catch forceps till other steps in the operation are completed. From one to two more such sutures, placed about a half-inch apart, are taken (Fig. 1).

The next step in the operation is the introduction of the closing sutures, which are of silkworm-gut. Before closing the abdomen the parietal peritoneum and the peritoneal covering on the anterior wall and the anterior portion of the fundus are scarified with a bistoury.

The peritoneum is closed by a running suture of catgut, as is indicated in Fig. 2. The order of tying the sutures is: 1. The running suture in the peritoneum. 2. The kangaroo tendons (Fig. 3). 3. The silkworm-gut closing the walls *en masse*. In the execution of this operation care should be taken not to attach the uterus too high up, so that the tension will not be too great; also, to guard against too deeply piercing the uterus with the needle, for fear of going into the cavity of the organ.

It is shown by the preceding text, as well as by the figures, that the uterus is attached to the abdominal wall by the approximation of the anterior portion of the fundus, and not by the posterior wall. This is to avoid the formation of long bands of adhesion for the entanglement of bowel and omentum, as is apt to be the case in the so-called improved or modified operation. The advantages to be gained by this operation are:

(*a*) It holds the organ in a more natural position, and long and dangerous bands of adhesion are not so apt to form, for the reason that the intra-abdominal pressure is not directed at right angles to the long axis of the uterus.

(*b*) The omentum falls down behind the uterus (Fig. 4) instead of in front, as is the case with the modified operation.

(*c*) The stitches are carried into the wall, gathering up the fibrous structure of the aponeurosis or sheaths of the recti muscles, and are not as apt to lengthen out.

(*d*) On account of the formation of shorter bands, the retrodeviation or prolapse is not so liable to recur.

In regard to the other apparent advantages of ventrofixation over other operations to correct retrodeviations or procidentias, it is seen that it permits, by the opening of the abdomen, of a thorough exploration of the uterine adnexa and the removal of diseased structures that cannot be accomplished with the same facility in other operations, except the intra-abdominal shortening of the round ligaments and the operation of tucking them up as is devised by Ruth. The chief objections are: 1. The complications liable to arise in subsequent pregnancies and deliveries, which, however, have not occurred after my operations. 2. The formation of dangerous bands for the entanglement of bowel and omentum, which is reduced to the minimum by the anterior attachment.

These must, in all candor, be admitted; but when we consider the failures that are apt to follow the Alexander operation, together with resulting pathologic changes in other operations, are we not justified in the continuation of the practice of anterior ventrofixation, not ventrosuspension or ventrosuspensio uteri?

TWO CASES OF DYSTOCIA FOLLOWING VENTROFIXATION, ONE REQUIRING CESAREAN SECTION.

By X. O. WERDER, M.D,
PITTSBURG.

Among quite a considerable number of ventrofixations performed by me I have accurate knowledge of at least five patients who subsequently became pregnant and were delivered at term. Three of these had normal gestations and labors. In one, not strictly belonging to this class, in which the uterus was fastened to the abdominal wound after myomectomy, with extraperitoneal treatment of pedicle by means of an elastic ligature, the fundus sloughed, resulting in an abdomino-uterine sinus which persisted during pregnancy, and through which there was a periodical, more or less profuse bloody discharge from the placenta. The patient was delivered of a living child three weeks prematurely; the child survived and the mother made a good recovery. The fifth case is one of the two cases of dystocia which I am about to report.

Case I.—Mrs. M. Z., aged thirty-seven years; seven children, last one ten years ago, which was stillborn. She was admitted to Mercy Hospital, March 15, 1896, with history of menorrhagia of ten-years' standing, menstruation lasting a week or more, leucorrhea, constipation, headache, backache, and bearing down. When in erect posture a large mass protruded from the vagina.

On examination the uterus, with anterior and posterior wall of vagina, was found protruding from vulva, there being an almost complete inversion of genital canal; os was patulous, cervix eroded, perineum lacerated. March 19, 1896, the following operations were performed at one sitting and in the order enumerated: Curettement; anterior colporrhaphy (Stolz's operation); median celiotomy, two-inch incision; ventrofixation, fundus being anchored to muscle and fascia at lower angle of wound by three buried silkworm-gut sutures

passed through posterior surface of the fundus just below insertion of the tubes; Emmet's colpoperineorrhaphy. Convalescence normal; discharged April 20, 1896.

On Sunday, September 18, 1898, my assistant, Dr. F. F. Simpson, was called to her in consultation by Dr. Potts and learned that labor had begun about a week before, and that, according to her statement, amniotic fluid and meconium had escaped at that time. Pains were persistent, though not uniformly severe, till Thursday night, when they became stronger, and the uterus remained in a condition of firm contraction thereafter. Temperature became elevated, pulse 110 to 120. Sunday night she had a chill, temperature reached 103°. It was then decided to take her to the hospital. When seen by me on Monday morning, September 19th, temperature was 99.8°, pulse 120, but good; general condition good; she had had few pains during the night. Her uterus was in firm tetanic contraction, spherical in shape, extending to mid-point between umbilicus and xiphoid, fundus protruding anteriorly to a marked degree. Fetal head recognized with difficulty to right of and on line with umbilicus, but the uterus was too firmly contracted to outline position. By the usual vaginal examination the cervix could not be reached, but above the promontory of the sacrum a thick, firm mass protruding into the anterior vaginal wall was felt. Under chloroform the cervix was found to be posterior and eight inches from the vulva; repeated efforts to draw it down were of no avail. There was no evidence of fetal life; the vaginal discharge was much discolored and offensive.

In view of the prolonged labor and the evidence of septic infection, as well as the firm tetanic uterine contraction threatening rupture; the high position of the cervix, which was not dilated above the size of a silver dollar, and which could only be reached under anesthesia with the whole hand introduced into the vagina; the futile attempts to bring it down or to reach the fetal extremities through it; in addition to the hard, tumor-like mass behind the symphysis, evidently the imprisoned anterior wall of the uterus obstructing the pelvic canal—the only solution of the difficulty seemed to be Cesarean section. She was, therefore, prepared for operation. The last examination was made between 10 and 11 A.M. At 12 o'clock the pains became more severe and frequent; at 2 P.M. she was anesthetized and placed upon the table for section. When the vagina

was being cleansed on the operating-table the breech was found presenting at the pelvic outlet. She was, therefore, transferred into another room, where the dead fetus was rapidly delivered. The patient was septic, and had more or less elevation of temperature for over a week, but she finally made a good recovery. Some of the gentlemen of this Association, which held its meeting at the time in Pittsburg, and who saw the patient on the operating-table, will no doubt remember this interesting case. How spontaneous delivery was accomplished at the last moment, in the face of apparently insuperable difficulties, is certainly a very interesting question. The only explanation which I could advance is that the very powerful uterine contractions present during the last two hours caused a rupture of some of the adhesions binding the uterus to the abdominal wall, releasing the imprisoned anterior uterine wall and allowing the cervix to descend into the sacral excavation. It was certainly a very fortunate termination of a case in which a Cesarean section, on account of the existing infection, might have proved very disastrous.

CASE II.—Mrs. P., twenty-four years old, married ten months; puberty at fifteen years; menses regular, of four days' duration. About five years ago she gave birth to a child; shortly afterward began to have considerable pain in the back and legs, menses rather profuse and painful. She had uterine procidentia, for which a surgeon of repute performed several plastic operations in conjunction with a ventrofixation four years ago. She left the hospital before her abdominal wound was healed. During her pregnancy she suffered considerable pain more or less constantly after the third month. She fell into labor at 10.30 P.M. of March 1, 1899. The pains were very strong, but ineffective. Examination by Dr. Rectenwald the next day under anesthesia showed cervix high up posteriorly, undilated; the fundus was down anteriorly. Several unsuccessful attempts were made by Dr. Rectenwald to bring the cervix down. He finally advised removal to the hospital for Cesarean section. She was admitted to Mercy Hospital March 2, 1899, at 7 P.M. Examination showed the abdomen distended to a degree expected from gestation at term, not symmetrically, however; the highest point of the uterus did not reach higher than the umbilicus. Apparently the long axis of the uterus was obliquely across the abdomen, the point of the fundus being about three and

a half inches above the symphysis and two inches to the right of the median line. The cervix was about on a level with the pelvic inlet and occupying a position to the left of the promontory of the sacrum. The long axis of the fetus was in the same direction, the head being in the lower segment of the uterus. Below the point of uterine attachment to the abdominal wall the abdominal parietes were very tense, and the relaxed abdominal wall above the attachment was closely folded over the point of greatest tension. The cicatrix was broad, irregular, and puckered at several points.

Vaginal examination showed the anterior lip of the cervix about six inches from the vulva and occupying a position in the pelvic inlet to the left of the promontory of the sacrum. The anterior wall of the uterus was firmly contracted, located behind the symphysis, and very thick, suggestive of a neoplasm, but its true nature was recognized. When admitted to the hospital no fetal heart-sounds could be heard and no movements felt, though Dr. Rectenwald was able to obtain both before removal to the hospital. Pulse was about 90, temperature normal, pains frequent, her suffering being very intense. There was evidently nothing else to do but a Cesarean section, as it was impossible to change the position of the cervix, situated to the left of the sacral promontory. Fetal parts could not be reached per vaginam even under anesthesia.

Operation at 9 P.M.; ether anesthesia; five-inch incision in the middle of the abdomen (which, however, was not the normal median line, for that was drawn to the right), beginning four inches from the symphysis and extending to the umbilicus; increased one inch beyond the umbilicus; the cavity was protected by pads packed around the uterus. Four-inch incision made through the uterus at a point a little to the left of the median line, beginning at about an inch anterior to the transverse line between the tubal ostia, and extending it three inches through the fundus into the posterior wall of the uterus, which was presenting. The placenta was found attached directly beneath the line of incision. It was quickly detached, the amniotic sac ruptured, the uterine opening enlarged posteriorly by tearing, the arm caught, and the child quickly extracted. The uterus was then rapidly delivered and the cervix grasped and compressed, thus preventing the loss of an ounce of blood. The child was then placed in the hands of an assistant and the pelvic cavity packed with pads. The

uterus was dissected loose from the abdominal wall in front, the torn attachments being very firm, extending over an area fully two and a half inches in diameter, involving even the bladder, the peritoneum of which was wounded and had to be approximated by catgut sutures.

The uterus was then carefully examined. The anterior wall was a large, raw, lacerated surface, marked by almost total absence of normal muscular structure, which seemed to have been replaced by connective tissue; it was at this area only one-third of an inch thick, whereas the remainder of the wall contracted to a thickness of one and a half inches. It would have been impossible to bring the peritoneal edges together, covering this extensive wound, without excising the whole uterine cicatrix. This was not thought advisable, partly from fear of infection which might have already occurred, partly from the condition of the patient, whose pulse was now 120, and more particularly because of the doubtful effect of such a procedure on future pregnancies. A Porro operation was, therefore, decided upon, the technique adopted being the Baer method of hysterectomy as practised by me for removal of uterine fibroids. The operation proved a very simple and rapid one. The patient made an uninterrupted recovery, and was able to leave the hospital at the end of five weeks.

In regard to the technique adopted in my four cases, excluding the myomectomy, followed by pregnancy and delivery at full term, it was that of ventrofixation in contradistinction to suspensio uteri, anchoring the fundus to the abdominal wound by two or three silkworm-gut sutures passed through muscles and fascia and through the fundus uteri, beginning at a line between the tubal ostia, the second suture about one-quarter of an inch down on the anterior uterine surface, including about one inch of uterine tissue. In the first case of dystocia reported this technique was somewhat changed, inasmuch as the first suture was passed between the tubal ostia and each succeeding one about one-quarter of an inch apart in the posterior wall of the uterus, the anchorage at the same time having been broader and wider than on previous occasions, for the reason that I was anxious to make a firm fixation to hold up the large, heavy uterus. This broad attachment, including a portion of the posterior aspect of the fundus during gestation, no doubt not only interfered with a proper expansion of the anterior uterine wall

and fundus, but they together formed in addition a tumor blocking up the pelvic inlet during labor.

In the second case I learned from the operator that the fixation sutures were passed through the anterior wall of the uterus near the fundus, but an infection occurred, resulting in very extensive, firm, unyielding adhesions between the abdominal wall and the fundus uteri.

The terms ventrofixation and ventral suspension, or suspensio uteri, are frequently used promiscuously. To avoid misunderstanding, I wish to say that by ventrofixation I mean an anchorage of the uterus to the abdominal wall by sutures which include parietal muscles and fascia—in other words, fixed tissues; while suspension is an attachment to the loose abdominal peritoneum and subperitoneal tissue only. The paper deals with the first method, or ventrofixation, exclusively. It is not intended to discuss the relation of the operation to dystocia at length, as this has been done very ably and thoroughly by writers abroad as well as in this country. My object in presenting this paper was not only to place on record these two very interesting cases, but also to invite a discussion on some points of great practical importance to every operator.

It would be neither fair nor just to use the cases just reported as serious arguments against ventrofixation. A careful study of them will show, as I will freely confess, that not the operation but the technique was at fault. In my own case my anxiety to secure the permanent retention of the inverted pelvic organs within the pelvic cavity led me not only to make a very broad attachment between the fundus uteri and the abdominal parietes, but also to include a portion of the posterior surface of the fundus in this anchorage. While this is perfectly permissible and proper in operations for uterine prolapse after menopause has set in, during the child-bearing period it becomes dangerous, as it allows of expansion of the posterior uterine wall only, and not of the fundus and anterior wall, which, though participating in the general hypertrophy of pregnancy, are held down by the sutures and block up the pelvic inlet during labor. A less broad attachment, as I have learned since, including only about half an inch of uterine tissue, or even less, with the sutures passed through the anterior uterine wall, the upper or first suture beginning at least half an inch below

the tubal insertion, would have been quite sufficient to fix the uterus permanently to the abdominal wall, provided the sutures had transfixed the fascia. Such a fixation would not interfere with the expansion of the fundus and a large portion of the anterior uterine wall. It would, therefore, avoid the dangers of dystocia.

In the second case the technique was faulty because the operation was followed by infection and suppuration, resulting in very extensive and unyielding adhesions, binding a large portion of the fundus and the whole anterior uterine wall firmly against the parietes.

It is, therefore, evident that in order to prevent such distressing consequences following this operation during the child-bearing period a very careful and exact technique is essential. A searching study of the cases of dystocia reported as due to ventrofixation would probably discover just such flaws in the technique as have been described in these two cases. Instead of being an argument against ventrofixation, these cases, therefore, merely demonstrate the necessity of greater care in the performance of this operation, as well as a more careful selection of the cases for it.

This brings me to the question, When should ventrofixation be performed? I will not discuss the relative merits of shortening of the round ligaments and uterine suspension for ordinary cases of retroversion and flexion; both have their advantages and disadvantages, but I believe one or the other of these operations quite sufficient for the relief of these conditions, with perhaps very few exceptions. Ventrofixation has a place, however, and a very important one, in my opinion, in the treatment of prolapse of the uterus, and especially in the extreme forms of procidentia or complete inversion of the pelvic organs. Neither Alexander's nor the intraperitoneal shortening of the round ligaments or suspensio uteri will hold up the uterus and resist its downward tendency to the same degree as ventrofixation. When combined with the necessary plastic operation on the cervix and the vaginal outlet, ventrofixation, in my experience, has proved the ideal operation for this extremely miserable condition. In some fifteen or sixteen cases of the aggravated form of procidentia uteri in which this combination of operations was performed, I have not had a single failure, nor, to my knowledge, a recurrence. I have only found it necessary to do hysterectomy in one case in which the condition of the cervix was strongly

suspicious of malignant disease, and in this I performed Fritsch's operation of vaginal hysterectomy with resection of the anterior and posterior vaginal walls.

Another class of cases in which ventrofixation is preferable to suspension or shortening of the round ligaments, are women suffering from posterior uterine displacements complicated by chronic metritis, with a marked enlargement of the uterus. The weight of the hyperplastic organ will soon draw out the suspensory ligament, after suspensio uteri, to such a length that it is no longer capable of holding the uterus up in ante-position; but in spite of this attachment, if it has not given way, it will sink back into its old malposition. The round ligaments also become stretched in the course of time as a result of the constant dragging upon them, and the uterus will recede more and more from its natural position behind the symphysis. A ventrofixation, however, when properly done, may invert a weak, flabby abdominal wall to a certain extent, as seen by me in one case, but it will remain in contact with the uterus.

In conclusion, I wish to say that while suspensio uteri and shortening of the round ligaments have largely taken the place of ventrofixation during the child-bearing period in the treatment of ordinary displacement, and very properly so, I believe the latter still the operation of election in the more aggravated forms of procidentia uteri, and also in those retrodisplacements of the uterus where the latter has become greatly increased in volume as the result of chronic metritis. These are the conditions usually accompanied by much suffering and distress, to the extent frequently of incapacitating women for work. Restoration to health and usefulness can be accomplished by ventrofixation and the necessary plastic operations in the vagina. These operations can be done even during the child-bearing period without fear of endangering future pregnancies, provided careful attention is given to a few important details in the operative technique, as pointed out above. The numerous cases of normal labor and spontaneous deliveries following this operation are proofs of this statement. The cases of dystocia reported are, in a great measure at least, the result of the faulty technique used, as has been shown in two cases presented to you in this paper.

A SIMPLE, EFFECTIVE, AND ESTHETIC OPERATION FOR SHORTENING THE ROUND LIGAMENTS.

By H. W. LONGYEAR, M.D.,
DETROIT.

THE object of this paper is to describe and commend to the profession an operation for shortening the round ligaments which the writer believes to be the best method which has been placed before the profession. The broad subject of retrodisplacements of the uterus and the various abdominal and vaginal fixation methods advocated for their correction will not be entered upon, as this operation is designed to accomplish only what Alexander proposed to do in his operation, but in a manner more expeditious, certain, and less dangerous to the patient and the integrity of the parts.

In calling attention to the operation there will be nothing original advanced by me excepting in some points of technique. The operation having proved eminently satisfactory in my hands, and believing, from the dearth of literature on the subject, that it is not generally understood or its true value appreciated, and that there is a crying need for some such method of shortening the round ligaments to supersede those of a more or less elaborate and mutilative character, is my excuse for presenting it at this time.

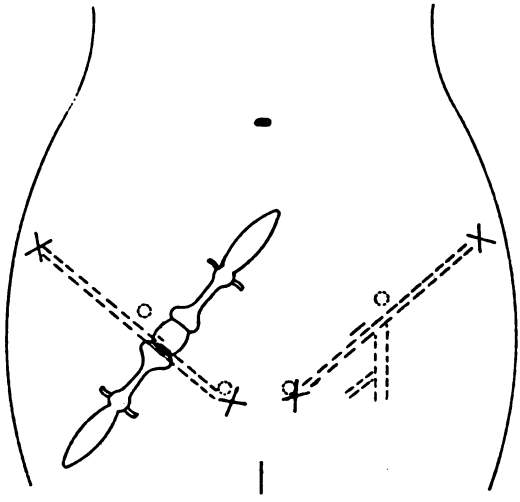
To some of you the operation is doubtless familiar. It was first brought to my attention by Dr. J. H. Kellogg, of Battle Creek, Mich., in a paper read at the annual meeting of the Michigan State Medical Society in 1897. Soon after this, appreciating the correctness of the theory of the method, I attempted it myself, but did not succeed in finding the ligaments according to his directions until I had seen him perform the operation. Its points of merit are, briefly, short skin incision; bloodless field; no mutilation of either ring and no slitting up of inguinal canal, and, consequently, no danger of future hernia; method of anchoring ligament accomplished by use of but one stay suture; no tissue is cut away; the

ligament can always be found; the operation can be made in a short space of time, and the patient can be allowed to get up sooner than with other more mutilative operations.

After the preparation of the patient, which should be by first shaving the pubes, and otherwise in every particular as for an abdominal section, the operation in brief consists of the following steps:

1. The incision. This is made about an inch in length (somewhat shorter in thin subjects) parallel with and just above Poupart's ligament, and with its outer end about half an inch inside of the

FIG. 1.



internal ring. It should pass through skin, superficial fascia, and fat, down to, but not including, the loose tissue which overlies the tendon of the external oblique. An incision of this length and situated as directed will be practically bloodless. If begun too far back, a branch of the superficial epigastric artery may be severed, and if too far forward a branch of the superficial external pudic artery will be encountered; while if carried beyond the layer of fat, veins will be cut, which will cause the field of operation to be more or less obscured. *The location of the internal ring is determined by the pulsation of the femoral artery, which passes just under it* (Fig. 1). (Dr. Kellogg locates it by measurement.)

2. The separation of the wound transversely by the use of eye-retractors mounted on long handles.

3. Clearing away the loose tissue overlying the tendon of the external oblique by pulling it from side to side, longitudinally with the wound, with blunt hooks, and catching with the retractors each successive layer as it is raised up by the hooks. The bloodvessels in this tissue are thus pulled back and held out of the field of operation. With the retractors holding back the tissues a field of about one inch square in size is presented, with the smooth, white, glistening aponeurosis forming the tendon of the external oblique at the bottom. Its oblique fibres can be usually easily traced to their junction with Poupart's ligament. By moving the retractors in various directions, being careful at the same time to keep the tissues within their grasp, a much wider field may be inspected if necessary.

4. Puncture, one-quarter of an inch in length, of the tendon of the external oblique, with the scalpel, at a point just above Poupart's ligament, and at the outer aspect of the cleared space (Fig. 1).

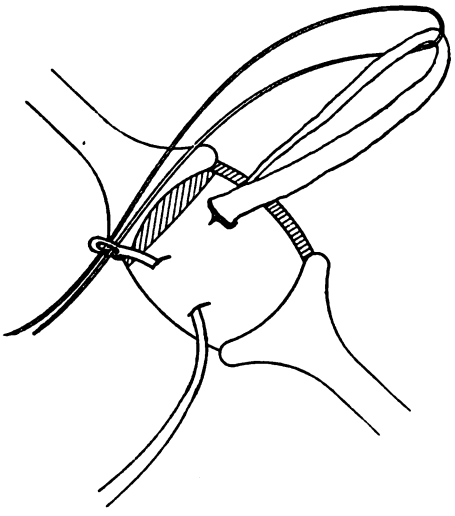
5. Insertion of the blunt hook, which should be passed downward close to the floor of the inguinal canal, then inward and upward, and the ligament with its surrounding investment of fat will be brought up out through the opening in the tendon.

6. Isolation and pulling out of the ligament, which is facilitated by stripping back the adipose and cellular tissue with blunt-pointed forceps as it emerges through the opening, and, if necessary, cutting with scissors any tendinous attachments which impede its freedom. Care should be observed during this maneuver not to cut the cuff-like prolongation of peritoneum which forms the canal of Nuck, and which is usually dragged from the internal ring and makes its appearance as the ligament nears the uterus. If the ligament still comes out easily its attachments can be carefully snipped with the scissors and the cuff rolled back.

7. Anchoring the ligament. This is done by Dr. Kellogg by means of silkworm-gut sutures which are passed from the skin surface, and which also serve to close the wound, although he also uses superficial catgut sutures for the latter purpose. I use one buried kangaroo tendon suture for fastening the ligament. Otherwise I follow the directions of Dr. Kellogg. The loop of ligament,

which will be from two to four inches in length, is held up by the assistant while its proximal portion, as it emerges through the opening in the tendon, is made fast to this structure by the suture, which is passed first through the aponeurosis, then the ligament, so as to grasp about two-thirds of it, and then the aponeurosis on the opposite side, and left untied. An aneurism needle is now passed in and out through the aponeurosis at the inner aspect of the wound, and through these two openings the loop of ligament is drawn by means of a heavy silk thread (Fig. 2), turned backward

FIG. 2.



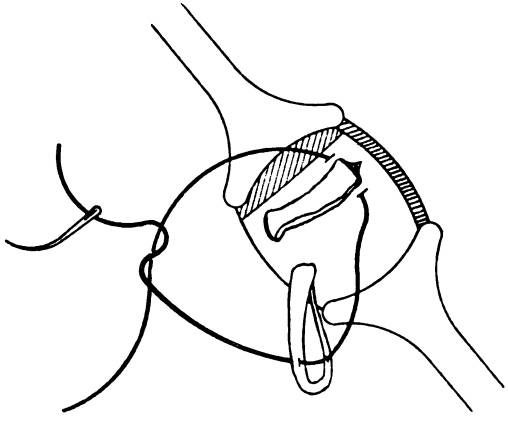
upon itself, and made fast to the aponeurosis and ligament by passing the loose suture through both sections and tying (Fig. 3). The one strand of kangaroo tendon is thus made to pass through the ligament three times and fasten the bunch together to the aponeurosis, in which position nature will soon make permanent fixation.

8. Closure of the wound. This I do with fine catgut, using the buttonhole stitch, after which the wound is sealed with iodoform-collodion, reinforced with a layer of gauze which is saturated with the collodion, thus making a firm and impervious seal. Before using the collodion the surface of the skin where it is to be applied

should be bathed with sulphuric ether, which removes any oily material and thus insures the firm adhesion of the seal.

A thick pad of gauze and a T-bandage complete the dressing. The seal is removed after the tenth day, and as it is stripped off, the catgut sutures, which by that time are absorbed beneath the surface, come away with it. The time required by this method of operating is very much less than by the Alexander method. If anatomical points are carefully noted, the ligament will be seized at the first trial and the operation quickly completed. Delay usually results from inability to properly locate Poupart's ligament. Twenty minutes should usually suffice to complete both sides after the operator has become master of the technique, and in some cases

FIG. 8.



it can be done in considerably less time if the operator is especially dextrous. The patient should be kept in bed for two weeks, which I think is sufficient time for the ligaments anchored in this way, by being woven in and out through the aponeurosis, to become firm. In the old method, where the ligament was cut off and sewed to the pillars of the external ring, the danger of the ligaments giving way is much greater, and requires the patient to lie in bed for a much longer period. The danger of the operation proving a failure because of suppuration is less with this operation, as the long loop of ligament could hardly be drawn back through the aponeurosis, while a severed ligament held only by sutures would easily give way.

Previous to operation a suitable pessary should be fitted for holding the uterus in anteversion during the operation, and this should be left in position, with occasional inspection, for three months afterward.

E. C. Dudley, in his recent admirable work on *Diseases of Women*, advocates this method of operating, modifying the technique only in the method of securing the ligament, which he accomplishes by passing the ligament back through the puncture in the aponeurosis, then bringing it out again at a point as far forward as possible, laying it back and making it fast to the aponeurosis with buried catgut sutures, after closing the first puncture in the aponeurosis.

My modifications of Dr. Kellogg's method are in the location of the skin incision by the relation of the femoral artery to the internal ring, suturing the ligament with a buried tendon suture, and closure of the wound with catgut and collodion.

I have thus far during the last eight months performed the operation on fifteen patients, which is perhaps too small a number and too short a time on which to base statistics, so these cases will not be reported at present. But the experience with them and the results up to date have convinced me that this operation has come to stay, as it has the points of merit designated in the title of this paper, being simple in design, effective in results, and esthetic because it is a work of art and beautiful to look upon.

DISCUSSION ON THE PAPERS OF DRS. DORSETT, WERDER, AND LONGYEAR.

Dr. D. Tod Gilliam, of Columbus.—With reference to the paper last read, I wish to say that I have become a little bit dissatisfied with the results of ventrofixation as a general operation. I am not satisfied with the overlapping of the ligaments on themselves within the pelvic cavity, for the reason that we have to depend on the weak and attenuated distal end of the ligament to hold the uterus in place. Being very desirous of learning some good and easy method of restoring and holding the uterus in its normal position, I made a trip to Battle Creek, Mich., to see Dr. Kellogg do the work. When I got there I found that he had just returned from Europe and was very

busy, having a lot of work on hand. I announced my coming; my train was late, and he courteously held the only case he had suitable for this operation until I arrived. I saw him do the most beautiful piece of surgical work I have ever seen; I was perfectly delighted with it. The operation that Dr. Kellogg does is practically what has been described by Dr. Longyear, with the few modifications he speaks of. After seeing Dr. Kellogg do the operation it seemed to me that I could go home and do it nicely and expeditiously; yet I had few misgivings, for the reason that theoretically I had supposed I could do other things nicely and easily, but found it different when it came to the practical test. I operated on my first case shortly thereafter, and it happened to be a very pale, anemic woman, who had been bedridden for a number of years. She had very little recuperative power and little vitality. I put her on the table, made an incision on one side, grouped around a little, and brought out the ligament. I was much pleased with the ease with which I caught it. I went to the other side, made a similar incision, grouped around and around, could not find the ligament, and finally became impatient. I then slit up the canal throughout its extent and, after considerable trouble, secured the ligament. I then fixed everything up, put the patient to bed, and the case has been suppurating ever since. It is several months ago since the operation was done.

Not long after I had another case; put the patient on the table, and fully intended not to have suppuration occur in this case if I could help it. I went to work, opened up one side, searched for the ligament, and finally found it. I then went to the opposite side and searched for the ligament, and did not find it. I did not pursue my investigation very long, so I fixed up the uterus with one ligament and left it that way. That is my experience.

I have two cases upon which I am going to operate when I go home. I shall approach them with more or less fear and trembling, because I may not be able to find the ligaments very easily. I shall make an earnest effort to do so without prolonging the operation too long, and if I cannot find the ligaments within a reasonable length of time I shall do a ventrosuspension, because both cases require operative procedure. After seeing Dr. Kellogg do the operation one is apt to think that it is just as easy as falling off a log. In the case on which I saw him operate he picked up the ligament in less than two minutes without any trouble whatever. An important point is in locating the spot for the incision. Dr. Longyear's method of locating it at the intersection of the femoral artery and Poupart's ligament will help us a great deal, providing there is no variation in the relation of these

parts. It is very important, according to Dr. Kellogg, that this should be a bloodless operation. The dissection is made with great care, so as not to wound a vein. Dr. Kellogg uses blunt hooks and little retractors for pulling the tissues apart. By their use one is enabled to conduct the dissection without wounding a vessel. If the latter is done the resulting hemorrhage would discolor the tissues and frustrate any attempt on your part to locate the ligament.

Dr. Kellogg told me that a surgeon came on from New York to witness one of his operations; he returned home, and after a little while he reported having done the operation thirteen times, and during this number of operations he had fished up the femoral vein three times. The first time Kellogg did the operation it took him three hours to find the ligament.

With reference to the other papers, I was much pleased with the one on ventrofixation. I did not think the time has come for us to discard this operation. There are conditions in which this operation will not be as efficient as some other method. There is no objection whatever in many cases in breaking up adhesions and going outside and shortening the round ligaments according to the method of Kellogg, when we can do it as efficiently as he does. There is no wounding of the abdominal wall. Dr. Kellogg often put his patients in a rolling chair at the end of four days, and his results are excellent.

I shall not discuss the other papers, because there are gentlemen here who can do it to much better advantage than myself.

Dr. William H. Humiston, of Cleveland, Ohio.—I have been much interested in Dr. Dorsett's method of operating, because it so closely coincides with my own. I believe in ventrofixation because I have had some happy results from it. My method is not quite as complicated as that of Dr. Dorsett, for the reason that I use but one suture with which to hold the uterus; I insert it in the same manner that he does. In addition, I scarify the anterior surface of the uterus for about three-quarters of an inch, and the grip of my suture embraces three-quarters of an inch of the uterus. One suture is sufficient to hold it with this scarification, which will give quite firm union. You have to support the uterus for a short time with a pessary—at least I do. I do not believe in using separate rows of sutures, as I find my results just as good with the through-and-through as with the layered sutures. I have followed my cases for the first and second years after operation, and nearly all have come to me for an examination subsequently. I have kept careful records, and I do not have herniæ resulting from the through-and-through suture. The operation is a thorough one, and it gives excellent results. I formerly used the

Kelly method of putting the suture in the top of the posterior uterine wall and fastening it to the peritoneum; that will not give a firm support, and the uterus later is frequently found in bad position. I believe in ventrofixation, however, for the reason that where it is necessary to be done, very frequently it is likewise necessary to remove one or the other appendage, and you have a complete control of the situation when you open the abdomen.

In reference to the Alexander operation, I have no use for it whatever; to my mind there are no indications for it. I have previously given my reason for rejecting this operation. Why do I find no indications for the use of the Alexander operation? If you have a fixed uterus it is contraindicated; you cannot make the operation. If you have diseased appendages and a retrodisplaced uterus, by doing an Alexander operation you will not relieve the case. If you have a retrodisplaced uterus without diseased appendages with lacerated cervix, by curetting and repairing the lacerated cervix, putting the uterus in position, draining and supporting it eight or ten weeks, you will get a permanent result, and the uterus will remain in place after the pessary is removed. I have demonstrated this time and again.

Dr. James F. W. Ross, of Toronto, Canada.—I have not yet made up my mind with regard to these operations. I am working in two fields. I do not care for the Alexander operation for the same reasons as those set forth by the last speaker, namely, that oftentimes there are adhesions in the pelvic cavity, or a fibroid tumor is present which can only be recognized on opening the abdomen, and I prefer, myself, to do an intra-abdominal operation. I do not meet with the cases that many others do, requiring these operations, or if I do meet with them, I do not consider that such operations are indicated.

I have my own particular views regarding misplacements of the uterus. A woman comes to me and for some reason or other requires an examination. She thinks she is pregnant; she is having amenorrhea, requiring an examination by the rectum. If I find a retroposed uterus in this case, I come to the conclusion that it is not such an abnormal condition as we so often consider it. In unmarried females I do not come across cases in which I consider it desirable to do them. There are two classes of cases that require these operations. One class is the hospital uterus—that is, the subinvoluted, retroflexed uterus, or the uterus weighted with a fibroid in the fundus; the other class of cases is that of procidentia. In one case upon which I operated for procidentia, within six months' time the uterus was down as badly as ever. In that case I did a ventrosuspension, not a ventrofixation, and I cannot see that my technique was at all different to that described by

Dr. Dorsett—that is, the abdominal wall was brought against the peritoneum with the uterus, and that would be one of the criticisms of the operation. I cannot see why it is the uterus should remain better fixed on account of sutures being put through fascia and muscle when the peritoneum is allowed to remain between the uterus and the other structures, unless buried suture is used. In the one case of ventrofixation for prolapse of the uterus, the uterus came down as badly as ever, and I was much disappointed. The adhesions were not strong. Buried sutures were used with silkworm-gut fastened to the fascia.

I have had a lamentable experience during the last year with intra-abdominal shortening of the round ligaments. It may be considered an operation that is free from risk, but I do not think so. I operated in one case for retroflexed uterus, fastened the round ligaments quite firmly, and the method I adopted was simply to pass a needle through the tissues below while holding the round ligament up in a loop and tying the transfixing silk around it. The patient did well for a day or two, then had a violent retching, and after this she did not do well. I came to the conclusion that something had gone wrong; her pulse went up. I passed down a silver probe, after taking out a stitch or two, and found the abdomen full of blood. I reopened the abdomen and found that the structures below the ligature had torn apart. The patient died shortly after reopening the abdomen.

This is a danger that may present itself to any of us in the operation of intra-abdominal shortening of the round ligament. If the ligament is made taut enough to hold a heavy uterus, even after a pessary is applied, during the retching the pessary may be forced out and the uterus may be again forced back into its position and the structures torn. I would like to ask Dr. Werder in his closing remarks to state definitely whether in these cases of ventrofixation he uses the buried suture, and whether or not he has had trouble with it. In doing the operation of nephropexy we find it necessary to split the capsule to bring the kidney substance in contact with the tissues above and to get the perirenal fat out of the way. If this is necessary in nephropexy, it is equally necessary in ventrofixation to bring the uterus to the front, so that it is uncovered by peritoneum, if we expect it to contract adhesions that will hold it in place. But the last word has not been said in reference to this subject. I am not wedded to any of these operations. I do not consider any of them as yet ideal. Many of them have done harm and numbers of them have been performed when entirely uncalled for. These operations are but seldom required, and when performed are seldom permanently satisfactory. They are far

from being triumphs of surgery, and will largely pass from among the operations of reputable surgeons.

Dr. Thomas B. Eastman, of Indianapolis, Ind.—I believe in ventrofixation and not ventrosuspension, and in order to get a good fixation, within the last two years I have been using for my fastening stitch heavy silver wire, which is passed through all the layers of the abdominal wall and brought out at the same position recommended by Dr. Dorsett. The parietal peritoneum is not united near the uterus. I leave this wire in from six to eight weeks. In this way I dispense with the necessity of a pessary, and have had only one bad result, and that was in a case where I was furnished with silver-plated copper wire instead of genuine silver wire, and in removing the dressings the wire was broken off and the uterus fell back further than it would have done had the wire remained the proper length of time.

Dr. James F. Baldwin, of Columbus, Ohio.—I believe that it was at the Niagara meeting of this Association that I was sat down on quite vigorously by several of the Fellows, because at that time I was advocating what was called the obsolescent method of treating these cases by ventral fixation. Some of the Fellows are changing their opinions if we may judge by the remarks which have been made. My technique—and I may say that I have operated on a large number of these cases, but I could not say how many—is a little different from that of Dr. Dorsett's, yet the result is about the same. My incision is a short one, just large enough to admit two fingers. My fingers are somewhat longer than Dr. Dorsett's. Through this short incision I can break up adhesions and then bring the uterus forward with two fingers. I do not try to get in my thumb; it takes too much room. I catch the fundus, if necessary, with a vulsellum. I then pass through a curved needle armed with silk, which serves simply as a guide for the iron wire which follows. This is passed through as Dr. Dorsett passes his—in front and just below the insertion of the tubes, so that I can anchor the anterior wall. The passage of the silk is followed immediately by the iron wire. My incision is made fairly well above the pubes, so that I may not wound the bladder. I pass my fingers to determine the upper boundary of the bladder. If we let this diagram (illustrating) represent the incision, I pass through at this point and again here (indicating) the handled needle, such as I use in closing the abdominal incision. This is passed through all the tissues, and the eye of the needle, which is in the point, brought up into view. My assistant passes into the eye one end of the wire, which is then withdrawn. I transfix again, and the other end is withdrawn. The uterus is now supported with the iron wire. With the same needle intro-

duced through the incision I scarify the anterior surface of the uterus for about half an inch in width. This is scarified just enough to get a little bleeding. I then scarify the corresponding portion of the abdominal wall, including the posterior face of the bladder. When I now bring up my wires these two raw surfaces are opposed to each other, and union of the peritoneal surfaces promptly takes place from the fundus of the uterus down to the bladder attachment. The ends of the wire are twisted over a little roll of gauze, so as to avoid irritation of the skin. If I have a hyperplastic uterus I probably put in two wires, so as to give more extensive support. The wire is removed at the end of about six weeks.

I agree with Dr. Ross, that no matter what method is used, we cannot fix the uterus to the fascia and muscles if we have, as in this illustration, the peritoneum interposed. This peritoneum will inevitably become somewhat loosened and result in a band of tissue holding the uterus forward, but not fixing it to the abdominal wall.

Dr. J. Henry Carstens, of Detroit.—Somehow or other I do not get the class of cases that most of the Fellows do—that is, I do not get cases of simple retroversion of the uterus that we can cure by an Alexander operation. I do not believe I get one such case a year. As a rule, I get old and tough cases, with such complications as diseased tubes, extensive adhesions, etc. In those cases we cannot make use of the Alexander operation. Simple cases of retroversion can be treated with success by the general practitioner by the use of tampons, pessaries, etc., especially those occurring post-partum. I do not see that class of cases. When I get a case it is generally a bad one. There is perhaps a diseased tube, and it is no use to undertake an Alexander operation for the relief of the patient. I find once in a while a case that will be relieved by a Mackenrodt operation, although I am not an advocate of it, because when the uterus sags down in the pelvis you do not lift it up and keep it supported by either a Mackenrodt or a Dührssen operation. If I should remove the diseased tubes and there is a retroverted uterus, and should sew the uterus up anteriorly, that uterus, as a rule, will undergo senile atrophy; and while it may stay in place by the operation itself and shortening the round ligaments, still what is the use of stitching up the uterus if it is diseased. There may be danger of malignancy; there may be a diseased condition of the mucous membrane and a badly lacerated cervix. Why should we try to save such a miserable uterus, sew up the cervix, and take out the tubes, and support the uterus? It seems to me that it is far better to do a vaginal hysterectomy in such a case, as I believe we would get better results, and the

danger of the operation is far less. Once in a while I have a case where there is one tube diseased; perhaps there is disease of one ovary and a tube; at the same time, it is a young woman and unmarried. We all practice conservative surgery in such cases; we take out the diseased tube and ovary, and we may find it necessary to resect the other tube and remove part of the other ovary in order to give the woman a chance to bear children. [Dr. Carstens then discussed the subject further with the aid of diagrams and cuts.] Personally, I have no use for ventrofixation. The cases are rare indeed where we are obliged to do this operation. If I have a case with a diseased uterus and tube, the disease being limited to one side only, I save the organs on the other side. I take the stump, sew it in the angle of the wound, or I shorten the round ligament inside of the abdomen, having the uterus resting in a normal position back of the bladder, with the uterus drawn over a little. I have performed such an operation a good many times. I had one case that seemed to get along all right, she having left the hospital, but a couple of days thereafter she had symptoms of intestinal obstruction. She was brought back to the hospital with obstruction of the bowels, and as a result of it died. A loop of intestine had got under at this point (pointing to diagram). This is the only case I have ever had where a loop of intestine got under this point of attachment. As this attachment stretches out we have an artificial round ligament. This can be easily remedied by scarifying the uterus anteriorly and posteriorly, so that it becomes firmly attached to the peritoneum. It seems to me this method, if I have made it plain to you, is a great deal more physiological than to put the uterus away up out of place where it does not belong, and where it may interfere with the functions of the bladder.

Dr. L. H. Dunning, of Indianapolis, Ind.—The old saying that the proof of the pudding is in the eating will apply very well here. As a matter of fact, the displacement of the bladder which occurs in these cases produces no disturbance whatever—at least, that has been my experience in the matter. The inconsistency of the Doctor's argument in favor of or against this will be shown in the limited number of cases in which he did the Mackenrodt or Dührssen operation. In those instances we have the bladder displaced more than in cases of operation by ventrofixation. We lift the bladder up and bring the uterus below it, in Dührssen's operation, and there results very little vesical disturbance. As a matter of fact, displacement of the bladder gives rise to little difficulty unless you pull upon it. If you have retroversion and the bladder is pulled down low into the pelvis, you get disturbances that are trying.

I believe with Dr. Ross, that the last word has not been said upon the subject. We are all of us anxious to find the truth and hold fast to it, and I have been through the stages that nearly every abdominal surgeon has, and have not formed a decided opinion yet, but in my belief the nearest to the truth is reached by ventrofixation, because it gives more permanent results, because it is attended by less evil consequences, and because, at least in my practice, it does not interfere with pregnancy. Out of quite a large number of cases, something over a hundred, that have come under my observation and care, there have been four pregnancies and deliveries without any difficulty whatever. For some time I practised the method of suspending the uterus, after Kelly's second method (you know he has given us three), by putting the ligature under the ovarian ligament and up through the abdominal wall. By that means my patients had more difficulty, and while in these cases I did not have genuine obstruction of the bowels, there were some symptoms of it, and sometimes I thought there was a hernia. There were colicky pains and difficulty in movement of the bowels. Then I tried the method of not suspending the uterus in that way, but sewing from the sides and bringing the ligature up on either side of the wound and tying it over a button. This method yielded permanent results, but I feared obstruction of the bowels. In my last cases I have used either ventrofixation or ventrosuspension. I have had a few unfortunate experiences with ventrosuspension—namely, suppuration of the buried stitches, and have had to remove them. I had no difficulty where I used two or three sutures in front as pictured in Kelly's book. I believe that we make a mistake when we introduce a stitch through the uterus back of the center of the superior surface of the fundus of the organ; it ought to be in front of the line and the scarifying of the uterus ought to be below the line of insertion of the stitch. These are the reasons why I would use ventrofixation—namely, it is more permanent in its results, it gives rise to fewer symptoms afterward, and does not interfere with pregnancy.

Dr. Willis G. Macdonald, of Albany, N. Y.—I would like to ask Dr. Baldwin whether he removes his wire sutures at any time after the operation.

Dr. Baldwin.—Yes, always. I do not bury the wire any more. I formerly did so, but I know better now. I take it out at the end of five or six weeks. It is fastened, as you understand, on the outside of the abdominal wall. It is not below the skin. When the woman is ready to leave the hospital I merely cut one end of the wire below the gauze pledget and pull it out.

Dr. T. A. Reamy, of Cincinnati, Ohio.—By the courtesy of the

Association, I will say a few words on this subject. I will admit that if shortening the round ligaments will answer the purpose of holding the uterus in position, it would be simpler and more satisfactory than ventrofixation or suspension. In recent years (for I do not do a great many of these operations now) I have spent much more time in the preparation of the patient for the Alexander operation, or whatever method I have adopted, than formerly. If there be disease of the tubes or of the ovaries, an abdominal section may be required. If the displacement or condition does not exist to a great degree, if the uterus is reduced in size, by a curettement and drainage and the wearing of a pessary by the patient for some time, after the uterus has been placed in its proper position, the woman may be made comfortable for months, and then by shortening the round ligaments by whatever method you prefer, or by doing a ventrofixation, you will find that a large number of the cases that are now abandoned as incurable will be cured. I fear the tendency of to-day is to do radical surgery too often, owing largely to the great facility with which it can be done by the masters of technique; hence, preliminary treatment is neglected. If we find we cannot effect a cure by mechanical means in some of these cases, then we should resort to surgical measures to cure them. The patient should also wear a pessary for several months after the operation for shortening the ligaments.

Dr. Frederick Blume, of Pittsburg, Pa.—I desire to call your attention to a point which is worthy of consideration. You are probably all familiar with the illustration in Kelly's *Operative Gynecology* of a suspended uterus, attached to the abdominal wall by two long bands. Now, there are many hundreds of women upon whom ventrosuspension has been performed, and who at the present time have such bands in the abdomen. I do not believe that any one of the Fellows present would permit an operation upon himself which leaves him in a similar condition, which, certainly, is not free from danger. It is difficult, indeed, to understand why obstruction of the bowel does not frequently occur under these circumstances. This is the only objection which I have to ventrosuspension.

The operation described by Dr. Dorsett is a ventrosuspension and not a ventrofixation, because the uterus is brought in contact with the peritoneum only. It does not matter whether the sutures include muscle and aponeurosis, or simply the peritoneum, union is obtained only between the uterus and the peritoneum, and as soon as the sutures are absorbed, or removed, or have cut through, if left *in situ*, the result will be a suspension. If a firm, broad union between the uterus and the abdominal wall is desired, the uterine wall must be scarified,

as mentioned by Dr. Baldwin. Thus the formation of long bands is prevented and the danger of hernia removed.

The position of the uterus in Dr. Dorsett's case is by no means the natural one. The normal position is a slight anteflexion, not anteversion, consequently the idea of attaching the uterus by placing the sutures on the posterior wall is correct, and would be preferable if such fixation did not result in serious disturbances during gestation and especially during labor.

The ideal treatment of retroflexion and version, in my opinion, is shortening the round ligaments, and especially the intraperitoneal operation, which is applicable to all cases, while the Alexander operation must be restricted to cases in which the uterus is freely movable and the displacement not complicated by diseases of the appendages.

In the case reported by Dr. Werder we meet with entirely different conditions, it being a complete prolapse of the uterus and vagina. I do not see what better could be done in this class of cases than repair of the vagina by plastic operations and a firm fixation of the uterus to the abdominal wall. When doing a ventrofixation for these conditions I attach the scarified anterior uterine wall to the abdominal wall and do not take the sutures out, as Dr. Baldwin, but leave them in.

Dr. John M. Duff, of Pittsburg, Pa.—The subject has been discussed so thoroughly in a general way that I shall not consume very much of your time, but desire to take up another line of thought in this connection, which has been hinted at this morning, and which would be disagreeable to the general body of abdominal and pelvic surgeons, namely—that operations are frequently unnecessarily done in cases of this kind. Perhaps they are by some men, but certainly not by discerning surgeons where they do not feel they are absolutely necessary. When a patient comes to us from a distance, and we do not have the opportunity to treat her for a time, sometimes the best thing we can do under the circumstances is a ventrofixation; but where we have adhesions which we cannot break up by applying force posteriorly, a great many of these cases of retroverted uteri can be cured by manipulation and massage of the uterus. Time and again I have had cases which have come under my care in whom operation had been advised, but by manipulation and careful position of the patient, keeping the bowels regular, preventing tenesmus, etc., I have relieved them very materially. Furthermore, in examining women for pathologic conditions, we must not always attribute their symptoms to retroversion of the uterus. There are hundreds of women whose uteri are retroverted and are living healthy lives; their uteri are not doing them any harm. There is one point in this connection

which I regard of great importance, and which is not practised very much, as far as I can learn, in this country, namely, the examination of women after labor. Every woman who has been confined should be examined at least forty days after her delivery to see whether the uterus is involuting properly or not, and if it is in proper position. If this were done, there would not be so many cases of retroverted uteri coming under our care. I hope the sentiment will go out from this Association of making it obligatory, having the laity to understand it, that it is necessary for physicians to make examinations before and after labor.

Dr. Dorsett (closing the discussion on his part).—I would like to call attention, first, to the application of the operation in suitable cases, and I am persuaded to do that after hearing the remarks of Dr. Reamy. I do not believe that Albert Smith or Hodge lived in vain. I believe that there is a field for the use of the pessary, and I do not believe that it is used often enough. It must be admitted, however, that there are cases that cannot be benefited by means of pessaries, and are amenable to nothing short of a surgical operation. I am led to make this declaration now, when I could not have made it years ago. We must know what gynecic surgery has been doing. Heretofore many patients died from inflammation and pyosalpinx whose cases are recognized today; so also we have an operation applicable at this day in suitable cases.

I neglected to put in the body of my paper the scarification of the uterus. I scarify the anterior portion of the fundus of the uterus as well as the parietal peritoneum, which makes fixation, and this will fully explain the remarks made by some of the other speakers.

In regard to the second case reported by Dr. Werder, in which he had to do a Cesarean section, it was a case in which there was suppuration. Occasionally we will get suppuration if we use either silver wire or silkworm-gut and let the stitch be tied on the abdominal wall externally. On the abdominal wall you are more apt to get suppuration than if you used kangaroo tendon and bury your sutures. That is my idea of it.

We all remember the picture in Kelly's work where we find the silk knot in one instance tied on the abdominal wall; in the other, on the uterine wall. That was the case in which the posterior uterine wall was brought up against the abdominal wall, and simply emphasizes what I tried to illustrate in regard to the principle of leverage. Where we have long bands that come up over the fundus of the uterus, in cases of pregnancy we have tension on those bands all the time; whereas, if we had it fixed anteriorly to the abdominal wall we

do not have the same amount of tension, hence the length of the bands should be remembered.

DR. WERDER (closing the discussion on his part).—I wish to indorse the views expressed by some of the preceding speakers in regard to the use of the pessary. I believe there is room for the pessary. We have all had cases that we could treat with a pessary instead of operating. If retroversion or retroflexion comes on after labor, the treatment will probably be successful; it will restore the uterus to its normal position, especially if combined with curettement and restoration of the vaginal outlet. But there are cases, even without any disease of the adnexæ, which cannot be cured by means of pessaries. I refer to cases of long standing. These require something more. The patients get tired of wearing pessaries, and they desire something that will enable them to get along without it; in those cases we have a perfect right to operate and to try and restore the uterus to its normal position by some operative procedure.

I wish to say a word or two in reference to the remarks made by Dr. Carstens, that he can get along without these operations. I would like to ask him what he does with cases of complete procidentia? How does he cure such cases without some such operation as has been mentioned?

DR. CARSTENS.—It depends a good deal upon the case. Once in a while I take out the uterus; occasionally I do a ventrosuspension, but this operation I rarely do.

DR. WERDER (resuming.)—The operation I perform is somewhat similar to the one described by Dr. Dorsett. I use to or three sutures through the uterus, include fascia, muscle, and peritoneum of the parietes. The material is silkworm-gut, which is buried and tied very tightly. In a large number of cases in which this technique has been employed I have had but few cases of suppuration following—possibly one or two.

DR. HALL.—What about the cases that have been operated on six or eight years ago?

DR. WERDER.—I suppose they still have the sutures in them. It has been said that these cases are simply suspensions; that there is no fixation, because the peritoneal surface of the uterus comes in contact with the peritoneal surface of the abdominal wall. I am not ready to answer that question. I will simply say this, that if you will examine such a case two or three years afterward you will find firm fixation, the uterus being firmly in contact with the abdominal wall. Whether the tying of the suture firmly causes pressure necrosis or pressure absorption of the peritoneum and brings the fundus against

the muscle of the abdominal wall, I do not know. I can simply say this, however, that there is fixation; and I again repeat, that if you will examine these cases two or three years after operation you will find the uterus as firmly fixed as it was the first month after operation.

Dr. Longyear (closing the discussion).—Dr. Reamy struck the nail on the head in regard to this matter, in that he has spoken, first, as a gynecologist, and, second, as an abdominal surgeon. In the first place, we should carefully examine and treat our cases to see which method is the most advisable one if we have to operate. If we can do as well by the patient without operation, it is clearly our duty not to undertake it. On the other hand, if an operation is required, it is likewise our duty to select that one which is the least dangerous to the patient and to the integrity of the parts, providing it will accomplish the object sought as well as one which is more dangerous and mutilative. Abdominal surgery is now so comparatively easy that we are in danger of submitting cases to the abdominal method that we really do not need to do sometimes, and as they usually recover without incident, very little is thought of the often unnecessary dangers which they are subjected to. Each one of you who has done a good deal of abdominal work, when you look back, will find one or more cases of the simplest variety that you have operated on in which your operation has been disastrous through some faulty technique either on the part of your assistant or yourself. Even the closure of the abdomen, often a simple examination, without doing anything beyond an exploration, results sometimes disastrously. I claim that by this operation for shortening the round ligaments many abdominal sections may be avoided and good results obtained.

Shortening of the round ligaments is not calculated to take the place entirely of other methods of fixation. The cases mentioned by Dr. Werder are especially applicable to ventrofixation. Cases of extreme procidentia cannot be cured by shortening the round ligaments. There is no question about that. Shortening the round ligaments, using nature's method of holding the uterus forward is best, instead of making some artificial ligament which will give rise to trouble. At every meeting of the Association during recent years we have had reports of cases where there has been great difficulty in parturition, or where there has been some stoppage of the bowel as the result of these operations of ventrofixation or vaginofixation. Dr. Ross mentioned a case where he simply shortened the round ligaments intra-abdominally, which was followed by a fatal issue. Supposing he had done the Kellogg operation, he could not have had such a disastrous result, even if

infection had occurred. He might have had failure in holding the uterus in position, but the woman would have been alive. Looking at the subject from a broad standpoint, we should select that method which will not only be of the best service, but also, if possible, that which will be the least dangerous to the patient.

In regard to the differentiation between fixation and ventrosuspension, I agree with one of the speakers that the final result in all cases of ventrofixation is usually suspension. You only have two serous surfaces approximated; the result is a constant pulling upon the peritoneum and elongation of the adherent portion; you may have cicatricial union between the peritoneum and the fascia under it, but that does not last long. You are sure to have ventrosuspension unless you have an immense amount of cicatricial tissue formed between the peritoneum and fascia, and you usually only get that in cases where there is a good deal of inflammation or suppuration.

In regard to buried silkworm-gut sutures, I have removed many of them from the abdominal wall that were put in by other operators, they having resulted in a good deal of irritation and suppuration, so that it is not my custom to use buried silkworm-gut in any of these cases. I use kangaroo tendon in cases of ventrofixation where I deem it advisable to do that operation. In Dr. Gilliam's cases of shortening of the round ligament the bad results followed a faulty technique, and I feel confident that as soon as he has acquired a sufficient knowledge of the operation and has become familiar with its steps, and has done it a few times, he will not have those troubles. He has told us that after seeing Dr. Kellogg do the operation he thought it was just as easy as "rolling off a log," but it is not until one has rolled off a number of times. I had a similar experience to his in one of my first cases in which I used this method, the first side being easy and the second difficult because of over-confidence. The sum and substance of the whole thing is to get the anatomical points right, then you will not have any trouble, particularly after you have done the operation a few times.

Some of the Fellows have spoken of the method of shortening the ligaments intra-abdominally. I think there is a great advantage in shortening them outside rather than inside. When you shorten them inside you will pull them from the outside inward, and your ligament suspends the uterus by its narrowest portion—that is, the portion which is attached to the tissues near the spine of the pubes, where it is extremely small in some cases, so small that you often cannot handle it without its breaking by the use of very little tension. Where you

shorten them extra-abdominally, there is much less liability of their stretching, as then only the heavy portion comes into use in supporting the uterus. Thus I think that the method of shortening the ligaments from the outside is by far to be preferred to shortening them inside, from a mechanical point of view as well as from that of safety to the patient.

EXHIBITION OF PATHOLOGICAL SPECIMENS, WITH DISCUSSIONS ON THE SAME.

X. O. WERDER, M.D.,
PITTSBURG.

GALLSTONES.

I HAVE a few specimens which will require only a few minutes to demonstrate. The first case is a large gallstone removed from a patient in May last by the operation of cholecystostomy. The patient had been in excellent health until the last child was born, which was followed by some pelvic disease, and the ovaries were removed in 1893. Her present trouble began in November, 1898. She had been constipated, had constant pain in the right hypochondrium, felt miserable generally, and had lost much flesh. Physical examination revealed a small, hard tumor about an inch and a half in diameter, which was found in the region of the gall-bladder. At the operation the gall-bladder was found firmly adherent to the omentum; the fundus of the gall-bladder had been perforated, and the apex of one stone found protruding through the opening. This opening was protected from the general peritoneal cavity by adhesions of intestines and omentum to the gall-bladder. I show you the large stone which was in the fundus of the gall-bladder. It evidently by pressure had brought on necrosis, which, in turn, caused adhesions and prevented disastrous results. The patient made a good recovery.

The next case I wish to report is also one in which I performed a cholecystostomy. The patient had been suffering from attacks of biliary colic for the last fifteen years, which lasted from four to five hours at a time, and consisted of pains beginning about the epigastrium. There was vomiting, cold sweat and chills at times, fever, with rapid pulse. Constipation was present. The attacks were

followed by soreness and the passage of large quantities of clear urine. Jaundice was present. There was an intense itching of the surface of the body. The last attack of jaundice lasted six weeks before the patient was brought to the hospital for operation. After opening the abdomen I found extensive adhesions around the lower margin of the liver, which it took a great deal of time to separate, and I had considerable difficulty in locating the gall-bladder, which was not larger than the tip of my thumb. In passing my finger back to the gall-bladder I found a firm, hard mass, feeling satisfied it was a gallstone. I then passed a probe into the gall-bladder, but could not get it to come in contact with the stone. So I finally incised the cystic duct and delivered this stone (exhibiting stone) with several small ones. I tried to pass sutures through the duct uniting the wound, but I found as soon as I tried to tie it it tore right through. The woman was very corpulent, and the operation was extremely difficult. The best thing to do under the circumstances was to pack in a large quantity of iodoform gauze, and leave it for four days. On the fourth day the patient had a slight chill, with elevation of temperature. I removed the gauze and found no discharge. I passed another strip of gauze, which I removed the next day, and the patient made a good recovery.

The third case was an ordinary cholecystostomy, and I report it on account of the large number of gallstones that were removed, there being from thirteen to fourteen hundred. They vary in size from the head of a pin to that of a hazelnut.

L. H. DUNNING, M.D.,
INDIANAPOLIS.

The point of interest in the case I desire to report is that the tumor formed by the distended gall-bladder varied in size. It was exceedingly large, reaching to the anterior superior spinous process at one time. It varied in size from day to day. Two stones were found after opening the gall-bladder. I show you the smaller one; the attending physician took the other one with him. The explana-

Adeno-carcinoma of uterus.

tion for the variation in size of the gall-bladder was a funnel-shaped opening, and this stone fell as a ball into the opening and closed it firmly. By change of position it allowed the escape of bile, then fell down again and closed the opening. We sewed the gall-bladder to the incision, inserted a drainage-tube, and the duct was found to be patulous. The only point of interest was the valve formed by the last gallstone.

WILLIAM H. HUMISTON, M.D.,
CLEVELAND.

Papillomatous Ovarian Cyst; Adeno-carcinoma of the Uterus; Sarcomatous Degeneration.

Mrs. H., aged seventy-one years and ten months, had borne four children, and during the last confinement the perineum was badly lacerated. She never was in good health afterward. During the last forty years she has had protrusion of the uterus. During the last ten years the procidentia has been more or less complete, and her distress has been proportionally greater. One year ago, however, she first experienced sharply acute pains across the lower part of the abdomen, and any attempt to replace the uterus gave her distress. The pain has been constant and so aggravated by walking or even sitting that she has been inactive for the past twelve months.

On September 15, 1899, under chloroform anesthesia, I removed the uterus and this large ovarian tumor by a combined vaginal and abdominal operation.

The study of microscopic sections of this tumor has been most interesting. The tumor is a papillary ovarian cyst undergoing a malignant degeneration of a mixed type. The more solid portion of the tumor is unquestionably a sarcomatous degeneration of the spindle-celled variety. Another section of the tumor shows beautifully that type of malignant degeneration which has been termed endothelioma, an intermediate stage between an epithelioma and sarcoma. We have, therefore, in this primary papillomatous ovarian cyst a mixture of spindle-celled sarcoma and an endothelioma. Section through the posterior uterine wall near the fundus also shows an adeno-carcinoma deeply involving the muscular tissue of the wall.

JAMES F. BALDWIN, M.D.,
COLUMBUS.

I have here a specimen which was handed to me by a physician of our city. He brought a similar specimen to me a month or so ago for examination. The patient is a woman, twenty-three years of age, who, so far as is known, is otherwise in excellent health. She passes a quantity of this kind of material from the vagina every two or three days. I do not know what it is, and I present it to you to find out. I suspected when I first saw it that it was introduced into the vagina by the woman herself, and I am somewhat inclined to think so still. Examination with the microscope shows vaginal epithelium and simply a mass without characteristic form. The material is not soluble in water, alcohol, ether, or chloroform. Caustic alkalies produce no effect upon it, neither do the strongest acids. When exposed it dries up, as in this specimen. If it is a foreign body which she herself places in the vagina, what is it? If it forms in the vagina, as she states, what is it? Her physician says that it seems to have a slight alcoholic odor when first removed, but this is soon lost.

JAMES F. W. ROSS, M.D.,
TORONTO.

Nana Pelvis.

I have a case to report upon which I operated about seven weeks ago, and I thought it would be interesting to the Society to show this photograph. (See Figure.) The case is one of nana pelvic dwarf condition (achondroplastic), very rare, but nowadays well recognized, the absence of cartilage between the epiphyses and diaphyses of the bone preventing the growth of the shaft and as a consequence shortening the ligaments. The patient had had two miscarriages. I happened to be in the country seeing another case when the doctor told me about her. I told him that the next time she was pregnant to let her go to full term and then we would resort

Nana pelvis.

to Cesarean section. One morning I received a telegraphic dispatch to come on the first train; the woman had been in labor since Sunday, and this was Thursday. He expected me to come and do a Cesarean section. I called counsel, and had pelvic measurements carefully made: Conjugate, 2¼ inches; transverse, 4⅛ inches. It was the smallest pelvis I have ever examined. We decided that it was impossible to effect delivery of the child by the pelvic route, and that the proper operation was a Cesarean section. This was done by removing the entire uterus through the abdominal cavity and approximating the wound above by two or three sutures, so as to bring the abdominal walls closely around the neck of the uterus. Warm towels were then wrapped around the parts, a rapid incision was made into the front wall of the uterus and came down directly upon the placenta. There was a great gush of blood. I pushed my hand inside, stripped off the placenta, and removed the fetus. The interior of the uterus looked greenish; the fetus was dead and macerated. I packed the interior of the cavity of the uterus with iodoform gauze, then stitched the wall with sutures, not going through the mucous membrane, then brought the serous membrane over, with mattress sutures, so useful in intestinal operations, approximating the surfaces so that the other sutures were buried in below. The patient made an uninterrupted recovery.

DISCUSSION.

DR. BLUME.—I would like to ask Dr. Ross whether he does not think a Porro operation would have been better in this case, as the woman was so long in labor—from Sunday to Thursday. I congratulate the doctor upon his success; nevertheless, I would like to have his opinion regarding the practicability of a Porro operation in such a case.

DR. ROSS.—I did not do a Porro operation because I prefer a Cesarean section in cases in which the patient's condition is good. In this case the uterus was perfectly normal, and I would prefer removing a portion of the tube under such conditions to prevent subsequent pregnancy. I would rather do a Cesarean section and take out a piece of each tube in such a case than do a Porro operation. I may say that I have done three Porro operations.

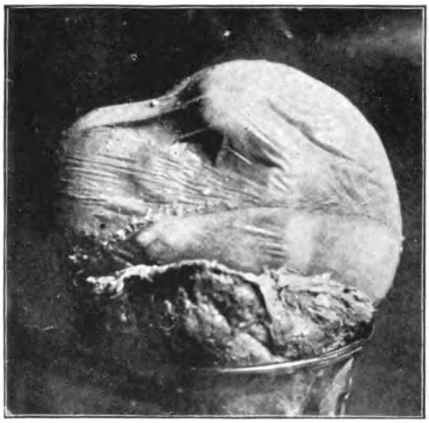
O. G. PFAFF, M.D.,
INDIANAPOLIS.

Interstitial Tubal Pregnancy; Case Operated in the Fifth Month, before the Occurrence of Rupture.

The case here reported is one of such rare occurrence that opportunity for extensive comment is lacking. Could a series of similar cases be collated, then, indeed, would the various side-lights reveal proper material for discussion. Detailed briefly it is as follows:

Mrs. A. S., aged twenty-two years, married three years, general health good, gave a history of several pregnancies, which had always resulted in abortion at early periods. At the time I first saw her she had not menstruated for months; the breasts were enlarged and contained colostrum, and she suffered from persistent nausea; she complained chiefly, however, of severe and almost constant pain in the right pelvic region. On account of severe pain medical aid was sought, and the attending physician, Dr. F. A. Morrison, recognizing an abnormal pregnancy, referred the case to me.

The local conditions were as follows: The os uteri was patulous; the fundus was easily located in the left side of the pelvis, and by conjoined touch no appreciable enlargement could be detected. In the right side was a large fluctuating body, which was evidently in some manner closely connected to the uterus. A sound was passed into the uterine cavity without difficulty, and measured a depth of three inches and demonstrated an empty organ. A diagnosis of tubal pregnancy was made and prompt operation urged. This advice was wisely accepted, and the patient was removed to St. Vincent's Hospital the same day, and upon the following morning I operated, assisted by Drs. Morrison, Wright, and Graham. Upon opening the abdomen a large reddish bag presented, which seemed to develop from the right wall of the uterus and involved the right tube, the fimbriated extremity of which occupied a position about four centimeters under the extreme right margin of the tumor, while the round ligament was given off at a lower plane. It was thus evident that I had to deal with a tubal pregnancy of the interstitial variety, and I at once determined upon its removal. In order to minimize the hemorrhage, as well as to secure the tumor in the abdominal



Interstitial tubal pregnancy.

wound, I clamped the upper portion of the right broad ligament, including the tube at a point where it tapered off from the bulging cyst, and also placed a long clamp to the left of the tumor obliquely across the fundus in such position that it not only shut off the entire left side of the cyst, but also included in its diagonal course across the uterus the right uterine artery.

I next incised the bag freely at its fundus and came directly upon the placenta. The muscular character of the walls of the bag was now perfectly evident, so that I did not hesitate to thrust my hand into the wound and turn out the contents of the sac—placenta, membranes, and fetus—intact. The fetus continued voluntary movements for a few seconds after extraction.

The incision into the thin walled bag was now partially stitched, the left clamp was removed, and the cavity was carefully explored. No opening whatever into the uterine cavity could be detected, and there was no point where the septum appeared sufficiently thin to admit of a successful attempt at vaginal drainage. I now removed the right clamp and placed a large glass drainage-tube into the cavity, closing the wound around it tightly with catgut and fastening it securely into the lower angle of the abdominal cut with two strong silk sutures. The utter freedom from hemorrhage is one of the remarkable features of the case. I attribute it in part to the contraction of the bag, but largely to the very firm compression of the left clamp, which no doubt crushed some of the vessels and favored clot-formation in others.

Drainage from the tube was quite free during the first days, but rapidly decreased during the succeeding forty-eight hours, and was so slight on the fifth day that I removed it. The subsequent recovery was uneventful, except that a small opening into the tube persisted for several months, when it closed over, only to fill up at each menstrual period, creating a sense of distention, which is relieved by pricking the pouting cicatrix, thus allowing the blood to escape.

I regard this case as unique from the surgeon's standpoint, and we must conclude that interstitial pregnancy is a rare occurrence, which fact is made more emphatic by the language of Lawson Tait, who says: "In the enormous experience I have now had of tubal pregnancy . . . this (a specimen presented to him post-mortem) is my solitary experience of interstitial tubal pregnancy, but it so closely resembles a number which I have seen in museums that I

take it to be quite typical of its class. I am, therefore, disposed to believe that from physical examination interstitial tubal pregnancy could not be diagnosed, and I imagine no symptoms which could help us to recognize it before rupture. The whole of the museum specimens of this class do not appear to amount to more than five or six. There is one in the Edinburgh College of Surgeons, one in the museum at Guy's Hospital, one in the museum at University College Hospital, and another in the museum of the College of Surgeons, described by Mr. Albert Doran."

I am now able to add to this very short list: one in the Medical College of Indiana. The accompanying photographic illustration shows the membranes to be still unbroken, and my specimen has the unique distinction of being the only one of its kind which was not secured post-mortem.

DISCUSSION.

Dr. James F. W. Ross.—There is one question I would like to ask the Fellows of the Association. I have been very much interested in the collection of cases of ectopic gestation occurring twice in the same patient. Since our last meeting I have had my second case of ectopic gestation occurring twice in the same patient, and if any of the Fellows have had or know of similar cases, I would be glad to have reports of them.

Dr. James F. Baldwin.—I have had one with eight months' interval.

Dr. John M. Duff.—I have had one case in which Dr. Werder and myself diagnosticated extrauterine pregnancy, and the patient refused operation at the time. She got better, and two years afterward had another development. She was operated upon at that time and both of them removed. The fetus was well developed in the recent pregnancy; in the other, microscopical examination showed detritus from the pregnancy, the fetus being macerated.

Dr. Rufus B. Hall.—I have had two cases of tubal pregnancy occurring in the same patient; in one case, I remember distinctly, at short intervals. I operated on a colored woman, removed one tube and a fetus, perhaps two inches long, three or four days after rupture had taken place. There was a pint or more of blood-clots in the pelvis. The patient made a prompt recovery. Ninety-one days later I operated on her a second time, forty-eight hours after rupture, and removed a fetus about two inches long. I remarked at the first operation that

it might be well to remove the tube and ovary, as I felt that a woman once subjected to an operation for tubal pregnancy would have tubal disease on the other side, which would justify one in removing these organs. In this case the organs were perfectly healthy in appearance, and I did not remove them. I think there are instances where we ought to leave a second ovary and tube, but I think when we do leave them the patient runs the risk of a second tubal pregnancy, as the clinical history in many cases shows. If a patient had marked tubal disease on the opposite side I would not hesitate to remove that tube, and it has been my practice to do so. Occasionally I have not done so, and on two occasions I have had to do secondary operations.

DR. JAMES F. BALDWIN.—In giving the details of my own case of tubal pregnancy I desire to say that the interval between operations was about eight months. The woman had been married fourteen years and never been pregnant. At the end of this time she became pregnant. I made a diagnosis of tubal pregnancy, and she was operated upon at once, the specimen being a beautiful one. Rupture had not taken place in this case. Eight months later I was sent for, she having missed a menstrual period, and I suspected tubal pregnancy. The character of the pain and her general symptoms were such as she had had in the first instance, and she herself was positive that she was pregnant and the pregnancy tubal. I again operated and verified the diagnosis of an unruptured tubal pregnancy. At the first operation the second tube had been found apparently healthy, and had, therefore, been left.

DR. O. G. PFAFF.—Excepting in the case I have reported, I do not know of a similar one in literature. I recall no other case of interstitial tubal pregnancy which had been operated upon before the rupture of the sac. The case I have reported seems unique.

DR. WALTER B. DORSETT.—I had one case which did not progress that far, in which I removed the entire mass.

EDWARD J. ILL, M.D.,
NEWARK.

TUBERCULAR KIDNEY AND URETER.

On April 27, 1899, I saw, with Dr. E. DeL. Bradin, Miss P., by occupation a seamstress. She had suffered for a year with more or less pain in the left lumbar region—not severe enough to inca-

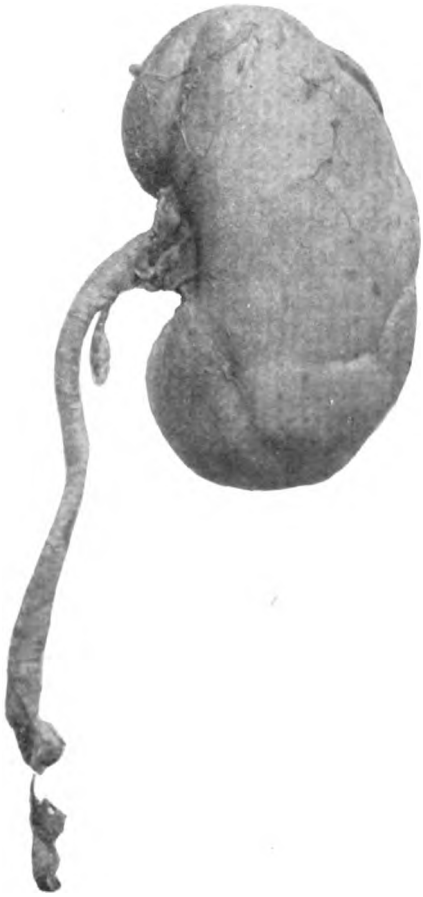
pacitate her until September 1, 1898. About February 1, 1899, she began to suffer with frequent and painful urination, lost strength and much flesh, passed water every twenty or thirty minutes during the day, and was up and down all night. The patient had been under Dr. Bradin's care but for a few days previous to my visit. He soon discovered that the urine was full of tubercle bacilli, pus, and connective tissue particles. The number of tubercle bacilli was astonishingly large.

An examination disclosed a very thick left ureter and several ulcerations in the fundus of the bladder. I utterly failed in catheterizing the right ureter, and, after several attempts, asked Dr. H. A. Kelly to tell me whether the right kidney was normal. He was kind enough to report to me that urine from the right ureter was entirely healthy. I wish to thank Dr. Kelly for this and other points brought out during a conversation about this case.

I proceeded to do the operation as suggested in Dr. Kelly's work, with the exception that the incision in the lumbar region was made nearly horizontal and 9 cm. in length. After the kidney, which was located exceptionally high up under the ribs, was delivered outside of the wound and the vessels ligated with a Grad ligature, the ureter was dissected out as far down as the finger and right hand would reach. A second incision, 6 cm. in length, was then made in the left semilunar line, as one would for ligation of the iliac vessels, leaving about 10 cm. of uncut tissue between the two incisions—an important factor in strengthening the abdominal wall. The peritoneum was pushed back until the ureter was reached. The enucleation of the ureter was then carried on until it was separated from the internal iliac posteriorly and the uterine artery anteriorly. A ligature was placed low down and another 1 cm. higher up, and the intervening tissue cut with the Paquelin cautery.

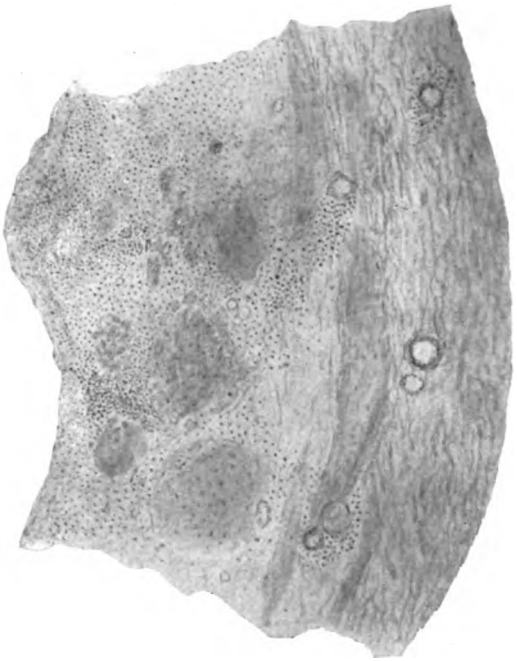
An incision 2½ cm. long, along the course of the ureter in the vagina, was next made, reaching down in the muscular coat of the bladder. A pair of forceps now aided to drag the lower stump of the ureter into the vagina, where, again, it was carefully dissected out of its bed in the muscular wall of the bladder, and then ligated with catgut. It was severed with the cautery. Whenever the cautery was used, it was for the purpose of destroying any diseased tissue. The wounds were all closed except where the Grad ligature emerged. The operation lasted one hour and forty-five minutes.

Fig. 1.



Tubercular kidney and ureter. × ⅔.

Fig. 2.



Section of tubercular ureter.

There was an immediate improvement in the character of the urine. The Grad ligatures were removed on the ninth day except one, which somehow got caught and came away on the fifteenth day. The patient was discharged in four weeks, about June 24th. It was my intention to remove the diseased portion of the bladder at any early date, but the patient's general health and the advent of the hot season did not permit.

During the last two weeks of the patient's stay at the hospital I injected one gramme of protonuclein in powder form into the bladder daily. This was continued by Dr. Bradin every two days, with all aseptic precautions, until July 20th, when tubercle bacilli could no longer be found; nor were any more found until August 29th.

Dr. R. N. Connolly, the bacteriologist, reports on this day that a few were again discovered, but that "the character of the bacilli present—that is to say, their size and staining properties—would indicate that the disease at the present time is not in a state of activity.

I was induced to use protonuclein because I had seen most favorable results on tubercular disease of the vagina following the extirpation of a tubercular uterus, by Dr. Barrow, of New York. This patient is now well since two and a half years.

On September 4th the patient reported herself as practically well; she had gained fifteen pounds in weight, passed water at normal intervals, and but once at night. The cystoscope shows a single ulceration in the anterior and upper portion of the bladder of about two or three millimeters in diameter. There are no tubercle bacilli in the urine passed at this day for such an examination. But evidently there is still tubercular disease of the bladder. The specimen shows a somewhat enlarged kidney, 11 cm. in length by 7 cm. in breadth, and a much enlarged and thickened ureter and narrow lumen. A tubercular cavity in the upper third of the kidney appears about 2 cm. in diameter and opens into the pelvis of the kidney.

Dr. F. R. Bailey, the pathologist, reports concerning the ureter, that it shows the presence of tubercle tissue throughout the entire thickness of the walls of the ureter.

Character of the Tissue. The tubercle tissue is composed almost entirely of the small round cells of the lymphoid type. Scattered among these small cells are larger cells derived from proliferation in the connective tissue elements and a few giant cells. Small areas of cheesy degeneration or coagulation necrosis are also found.

The Mucous Membrane. The epithelium is for the most part intact, completely lining the canal. There is some infiltration, with small round cells.

The stroma of the mucous membrane and the submucosa shows the lesion most prominently. Here there is an almost complete replacement of normal by tubercular tissue.

The internal muscular coat is broken up, its fibers replaced, or its muscle bundles separated by the tubercle tissue.

The external muscular coat is in the main intact, though tubercle tissue is found in the connective tissue between its bundles.

The bloodvessels show extensive changes. Their walls are thickened—this thickening being due partly to an increase in the connective tissue of the vessel-wall and partly to an infiltration of the wall with small round cells. The lumen is more or less constricted and in a considerable number of vessels completely obliterated.

Bacteriology. A few tubercle bacilli were found.

The specimen is a beautiful demonstration of an early stage of tubercular inflammation in the ureter.

RUFUS H. HALL, M.D.,
CINCINNATI.

I have here the specimen of a kidney removed some months ago. The history is interesting, and I will give a short report of it. The clinical history was typical of a patient suffering from stone in the kidney. For a period of six or seven years this woman suffered periodical attacks of nephritic colic. She had never passed a stone to her knowledge or that of her physician. She had occasionally bloody urine, but no great quantity of blood. Her physician visited her frequently and gave frequent hypodermic injections of morphine for the relief of colic. Some two or three months previous to operation there developed a tumor in the region of her left kidney, which increased gradually in size; at the same time she began to pass bloody urine. The attacks of pain now simulated obstruction from the time the tumor began to develop. There is one peculiar feature in the history of this case—namely, that from the time the tumor commenced to develop so great was the amount of blood lost that several times the physician was compelled to use a metal catheter to

Fig. 3

Microphotograph of tubercular ureter.

break up the blood-clot in the bladder to empty it. The woman was almost exsanguinated like a patient suffering from a bleeding fibroid. The tumor was two-thirds the size of an adult head when it was removed. The tumor resembled almost a perfect kidney in shape, and in it were found three or four stones, which I show you in this bottle, one being quite large and the others small.

DISCUSSION.

Dr. James F. W. Ross.—If it is in order I would like to have the opinion of the Fellows in regard to tubercular disease of the kidney. Most of you will remember that our text-books state that tubercular disease of the kidney is usually bilateral. It seems to me this is an erroneous statement which should be corrected, because I am satisfied from what I have been able to learn that this is not so, and in my earlier days I hesitated to remove a tubercular kidney on account of the above statement in medical literature. I have removed several tubercular kidneys and the patients have done well; they are living now and in good health.

Dr. J. Henry Carstens.—I want to indorse what Dr. Ross has said in regard to tubercular kidney. As a rule, the disease is not bilateral. I have operated on three cases. I never hesitate to operate. All of the cases have done well. One was the case of a young girl who had had a large abscess of the kidney. At first I opened the abscess to let out the contents; she was septic at the time, but under proper aseptic conditions she did well, and after six weeks I removed the kidney. Nine months later she got married, and since then has had a baby.

Dr. William H. Humiston.—I recall one case in which I removed a suppurating kidney. The patient was treated eight weeks for typhoid fever. She was brought to me, and after a careful examination I made a diagnosis of abscess of the right kidney, and on passing a ureteral catheter in the left ureter and collecting the urine found this kidney normal. At first we made a nephrotomy for drainage. Patient rapidly gained in strength, and after we did a nephrectomy. This was six years ago. The patient made a rapid recovery and gained forty pounds in weight, and is at present attending to her household duties.

Dr. Edwin Ricketts.—I recall doing a nephrectomy on a patient for tubercular kidney, who lived four years and died from pulmonary tuberculosis. I have a patient under my observation now in whom extirpation of the kidney was done two years ago. The patient was

five months advanced in pregnancy; she went to full term, was delivered without any trouble whatever, and is to be delivered this coming December of a second child. So far there is no evidence of any trouble in the other kidney.

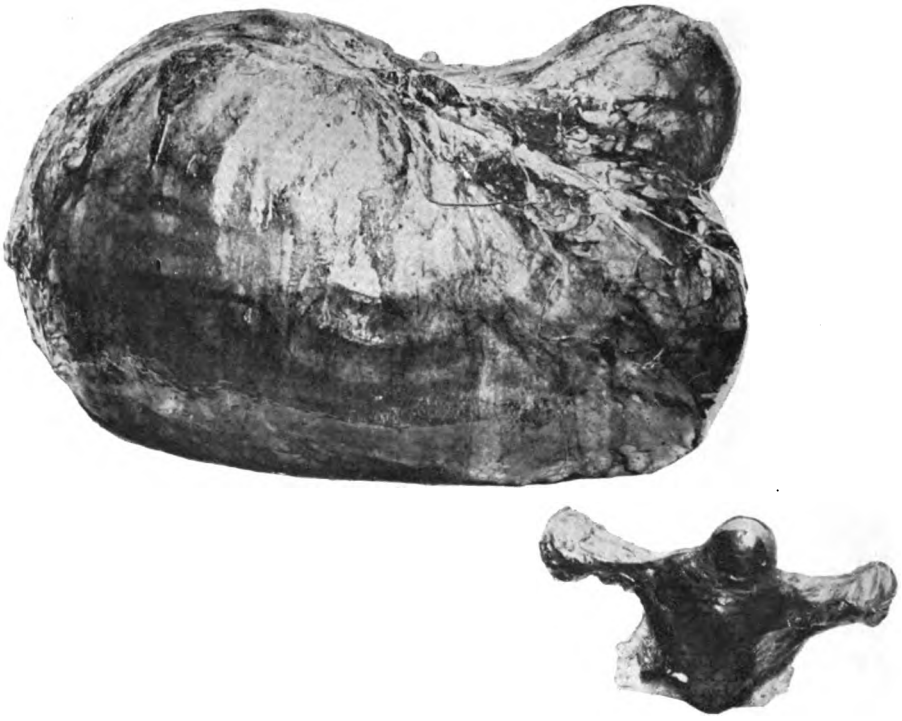
DR. L. H. DUNNING.—I have had experience with three cases of tubercular kidney, operating and removing the kidney in all of them. One patient died of shock within three hours after operation; the other two are still alive and enjoying good health at the present time. I also assisted Dr. Wishard in another case, the operation having been done a year and a half ago. The patient was reduced almost to a skeleton at the time of the operation. He made an excellent recovery, gained considerably in strength and weight, and is comparatively well today, now some three years after the operation.

From the little experience I have had I am under the impression that we frequently have cases of tubercular kidney where the disease is unilateral. However, I believe we ought to examine the urine upon the other side in every instance.

DR. WILLIS G. MACDONALD.—In the *American Medical Quarterly*, June, 1899, of which the Secretary of this Association is editor, I went into this matter of tuberculosis of the kidney pretty thoroughly from the standpoint of our own experience and of literature. I think we will find in a number of cases in which tuberculosis of the kidney has been demonstrated, with secondary tuberculosis of the bladder, that the bladder-wall itself is most resistant to tuberculosis, and that it frequently heals spontaneously. I believe that it is possible for the bladder to heal under the improved condition of the patient with tuberculosis without the use of the protonucleins.

Another interesting proposition presents itself, and that is as to the propriety always of removing at the first operation a tubercular kidney, particularly if it is very much enlarged and has a number of different pus foci. It is a question whether it is better to drain for a little time until the thing cleans up and then subsequently do a nephrectomy, or whether it is best at the first operation to remove the kidney. Our experience has been somewhat in the direction that has been indirectly alluded to here, that it is far better. We know that a considerable accumulation of pus is associated with tubercular kidney, and that by careful irrigation and drainage the condition of the patient may be very much improved, so that after a few weeks we can better remove the kidney instead of undertaking its removal at the first sitting.

DR. JAMES F. BALDWIN.—I can recall more than a dozen cases of tubercular kidney upon which I have operated, and I am of the



Fibroid tumor of the uterus.

impression that when we see these cases early we are quite safe in removing the diseased kidney; but when the disease has existed for a considerable time, in my experience, the other kidney, with only one exception that I recall now, has also been affected. I recall one case in particular in which I operated at the request of the attending physician, a prominent professor in Columbus. I cut down upon the right kidney, since he was positive that the left was normal. It was a male patient. On bringing the diseased kidney out so as to carefully examine it, and incising it freely, I found four or five distinct tubercular abscesses, but there was so much healthy tissue left, notwithstanding these abscesses and the enlargement seemingly due to the healthy tissue rather than to the diseased, that I told the doctor I did not dare remove it, since I was in doubt as to the condition of the other organ. I therefore returned it, putting in gauze for drainage. The young man died of uremia within a week, and at the autopsy I found that the left kidney (it was the right one that I had incised) was merely a sack of smear-case, so to speak. It had been absolutely of no use as an excretory organ for a number of years.

I had a similar case in which the right kidney was the seat of four or five abscesses and the left kidney was a mass of cheesy material, with no history whatever of disease pointing to the left side. I have a case now under observation in which I operated according to the suggestion of Dr. Macdonald, incising the kidney first and then draining it, then delaying the more radical operation. There is still some discharge from that side, and after I return from this meeting I shall examine carefully with a view to more radical operation.

EDWIN RICKETTS, M.D.,
CINCINNATI.

Fibroid Tumor of the Uterus.

I have some photographs which I desire to pass around in connection with the report of this case. The patient, Mrs. J., aged fifty-three years, consulted me for the removal of a tumor of four years' duration. The uterus was normal. The fibroid tumor was found within the left broad ligament, with no pedicle. An abdominal hysterectomy was performed November 14, 1898. The patient died in thirty-eight hours after operation. The tumor weighed sixty-five pounds. The growth originated in the left broad ligament, and was easily shelled out.

RUPTURE OF THE PUERPERAL UTERUS.

By JAMES F. W. ROSS, M.D.,
TORONTO.

It is not my intention to discuss fully the subject of rupture of the puerperal uterus. I intend to throw together a few rambling notes in order to bring the matter up for your discussion, knowing well that oftentimes active discussions are more valuable than the papers discussed.

It is a well-known fact that rupture of the uterus may occur before labor or during labor. The diagnosis of rupture of the uterus before labor must be extremely difficult; it is a subject about which I have no practical knowledge. The treatment of such a condition must depend upon the progress of the individual under discussion at the time. Rupture of the uterus during labor is, however, something with which every practitioner should be familiar, not, I trust, as a result of actual experience, but as a consequence of a close study of the subject in the class-room.

The causes of rupture of the uterus are well known. It may be well to mention them: 1. Mechanical impediment to the course of labor, from whatever cause. 2. An increase in the size of the child to be delivered, from any cause. 3. Faulty presentation. 4. Undue compression of the uterine structures between the descending fetal parts and the pelvic walls. 5. Operative violence. 6. Unnatural violence of the uterine contraction, due either to the administration of ergot or morbid excitability of the organ. 7. Straining at stool. 8. Disease of the wall of the uterus itself, such as that produced by cancer, by atrophy, by softening, by fatty degeneration, by hydatid disease. 9. Unnatural narrowing, rigidity, or atrophy of the os uteri. 10. Abnormal development of the uterus. 11 (one that should come under the first class, of mechanical impediment to the course of labor, but is often put in a class by itself). Deformity of

the pelvis. The operative violence more responsible for rupture of the uterus than any other is turning after the liquor amnii has escaped for some hours.

One of the cases of rupture of the uterus to which I was called was rupture produced by turning after the liquor amnii had escaped for some time from a primiparous uterus. The other operations required for the delivery of the parturient woman are occasionally the cause of rupture of the uterus. Taking all causes into consideration, version, under the circumstances just mentioned, produces 50 per cent. of the ruptures of the uterus met with. Rupture of the uterus is said to occur in about one in three thousand deliveries.

The question as to whether rupture of the uterus occurs more frequently in first or in subsequent labors does not seem as yet to have been settled. Some claim that rupture of the uterus is more frequently met with in primiparous than in multiparous cases. Others claim that it is more frequently met with in multiparous than in primiparous cases. Some state that rupture of the uterus may take place before the membranes have been ruptured. The duration of the labor has a great deal to do with the production of rupture of the uterus. The organ seems more prone to tear after it has become tired out by continual and prolonged contraction.

The fundus is less frequently ruptured than the body, and the body less frequently ruptured than the cervix. The posterior wall of the lower segment seems to be the commonest site of rupture, and the tear is generally found to run toward the left. The tears usually extend over a distance of three or more inches. It may occasionally happen that the peritoneal layer is not torn through, and in one such case that occurred in Toronto the fetus was delivered into the layers of the broad ligament and the patient made an excellent recovery. But even when the peritoneum has not been torn the patients may die after a prolonged course of septic poisoning.

Cases of rupture of the uterus may be divided into four groups: First, those that are beyond hope from the first, that have the usual classic symptoms of rapid pulse, dyspnea, precordial uneasiness, nausea, vomiting and cold perspiration. Second, those cases that have, owing to delay, passed beyond the operable stage and have become ill with commencing septicemia and peritonitis, peritonitis and septicemia drawing attention to the case, and this extra attention revealing the fact that a rupture of the uterus has occurred and been present

without giving rise to any early symptoms to indicate its existence. Third, the class of cases in which rupture is not immediately fatal, in which it is early recognized, and in which the patients are in good condition for operative interference. And fourth, a class of cases that I believe do occur, in which rupture is never recognized, but in which septic symptoms develop without any visible cause. There is nothing to explain the onset of these symptoms,

In a case in which a very rapid pulse follows a fairly severe labor and peritonitis sets in, a rupture of the uterus must be considered when endeavoring to discover a cause for the abnormal course of the puerperium.

After leaving out the probable infection from the hands of the accoucheur, infection from latent gonorrhea, what have we that is likely to cause severe peritonitis after labor? May not unrecognized rupture of the uterus account for the onset of peritonitis in many of these cases? The child is liable to be stillborn in any case of severe rupture of the uterus. If the fetus has escaped into the abdominal cavity, that cavity has become contaminated by meconium, by vernix caseosa, and the mother is scarcely likely to escape a fatal peritonitis.

According to some such classification we must direct our treatment. After rupture of the uterus has occurred the contractions of the uterus cease in a large proportion of cases. In some cases blood pours from the vagina. In some cases retraction of the presenting part takes place. In some, sudden severe abdominal pain is noted, and if the fetus has been delivered into the abdominal cavity the fetal parts can be felt through the abdominal walls. Under such circumstances there will be, perhaps, increased movements of the fetus, followed by a cessation of such movements and a change in the shape of the abdomen and the uterus. The uterus and fetus each form a distinct tumor, and there is an intervening furrow that may be made out.

Intraperitoneal hemorrhage can often be diagnosed by percussion in the loins. I have diagnosed intraperitoneal hemorrhage by means of percussion in cases of ectopic gestation. The percussion sound may change, however, but slowly with the change of position, owing to the fact that the blood is clotted and that it changes its position more slowly than fluid does.

Since taking up this subject, in conjunction with lacerated and punctured wounds of the genital tract, before the Buffalo Academy

of Medicine, in January, 1898, I have met with two cases of rupture of the uterus.

CASE I.—I was called to the country to see a case in consultation. The patient was four months pregnant and had been losing blood, and was evidently threatened with a miscarriage. Unfortunately, students get the impression that it is necessary to empty the uterus for fear of septicemia as a consequence of the death of the fetus *in utero*. The doctor in attendance, in this case a young practitioner, called another to his assistance, and together they decided to dilate the cervix and empty the uterus. This was carried out under an anesthetic. The patient did not do well; complained of intense pain and burning in the abdomen; began to vomit. The temperature became elevated and the pulse rapid.

When I saw her she had an anxious expression of countenance and looked extremely ill. I was informed that there was a peculiar condition existing, and that the case must be one of bicornuate uterus, as there was a place to the left side into which the finger would readily pass. I decided, notwithstanding the condition of the patient, to make a thorough exploration under an anesthetic. The fingers were then passed into the vagina, and two fingers were passed out through a rent on the left side of the uterus just about the situation of the internal os. As soon as they were passed into the abdominal cavity grumous, foul-smelling blood exuded. The conditions were not favorable for the performance of a celiotomy. A nurse could not be obtained within less time than twenty-four hours, and the patient's condition was such as to scarcely justify abdominal operation. I cut off a portion of the tubing on a large rubber douche bag, stitched two pieces together, one much shorter than the other, and passed them up through the cervix, through the rent in the uterine wall, into the abdominal cavity. The outer end of the short tube remained inside the cervix, and that of the other tube came down and ended at the vulvar orifice. Through this I washed fluid from the douche bag, allowing only a gentle flow and watching carefully the return. Iodoform gauze was then packed around the tube into the vagina, but was not carried into the cervix for fear that the tubes might be disturbed at the first dressing. A nurse was immediately telegraphed for and arrived next day, and for several weeks the pelvic cavity was irrigated. The temperature gradually

fell, the pulse dropping with the temperature. The patient made a slow convalescence, but ultimately regained perfect health.

I met with another case similar to this, and reported elsewhere. The uterus was ruptured by the hand of a practitioner. A plate representing the rupture is to be found in *The American Journal of Obstetrics*, vol. xxxvii., No. 4, 1898. It might be inferred from the note underneath the plate that I had done a posterior vaginal section. This is not so. The abdomen was opened, gauze was passed down through the rent into the uterus, on into the vagina, and a drainage-tube was placed in the cul-de-sac of Douglas from the front. At the commencement of the operation the abdominal cavity was thoroughly flushed and all blood-clots were carefully removed. The gauze, passed through the rent on the posterior surface of the uterus, was cut level with the serous membrane covering it. The patient made an easy recovery.

CASE II.—Mrs. H., aged thirty-eight years. For the following history I am indebted to Dr. McPherson, who attended the patient and sent her into the hospital under my care. When the doctor arrived he found that the patient had been in labor for five hours. She had had several children. The pains were very severe, but there did not seem to be much contraction of the uterus. The os was dilated and the cervix soft; membranes protruding. On inquiry the doctor found that there had been no motion of the child felt for about a week. Child was in the first position.

Two hours after the arrival of the doctor, or seven hours after the commencement of labor, the patient cried out that she was dying, and complained of sharp pains in the left side. What little uterine contraction there had been ceased altogether. The patient was given chloroform, forceps applied, and the child was delivered without difficulty. The child was dead and macerated; had evidently been dead for some time. Before the forceps was applied the pulse was 120; it rapidly rose to 140, temperature 99°. There was no difficulty experienced in the delivery of the placenta. Though it looked green, it did not smell badly. About five hours after delivery, pulse 120, temperature 99°. Next morning, pulse 124, temperature 99°. Patient suffering from very acute pain in the abdomen. She was particularly tender on the left side.

As the condition did not improve a consultation was called, and it was decided to again place the patient under chloroform and ex-

plore the uterine cavity to ascertain the cause of the patient's condition. The temperature had now reached 101°.

Dr. McPherson passed his hand into the abdominal cavity through a rent of the cervix and vaginal vault on the left side. He found that his fingers were over the fundus of the uterus, surrounded by blood-clot. The consultant also made sufficient examination to satisfy himself of the existence of a ruptured uterus.

The pelvic cavity was then douched thoroughly with hot water and packed with iodoform gauze through the rent. The douche and the packing were repeated daily.

On the ninth day, or the third day after delivery, the pulse was 118 and the temperature 102.2°. The pain was very severe over the abdomen and required a large quantity of morphia to relieve it. Pains then came on in the calf of the left leg. These were apparently produced by cramps of the muscles. The condition continued about the same on the fourth, fifth, sixth and seventh days after delivery. On the eighth day the pain in the left leg became very severe and the leg became swollen; the veins appeared to be much distended, and they were inflamed. There was slight hemorrhage from the vagina, bright red in color, after the removal of the packing.

On the ninth day there was severe hemorrhage from the vagina. Pulse rose again to 120. Hemorrhage was checked by hot douche. On the tenth day pulse was 124, temperature 100°. On the eleventh day pain came in the calf of the right leg, and it was quite evident that phlebitis was now coming on in the right limb. Both legs were bound up in cotton and kept quiet. The patient remained in about the same condition for the twelfth, thirteenth, fourteenth and fifteenth days. As it was impossible to give her the necessary attention, she was moved into St. Michael's Hospital under my care.

She lay for several weeks with high fever, rapid pulse, and all the evidence of a profound septicemia. As she had double phlebitis, it was necessary to put her on a water bed. I despaired of her life, and on several occasions thought she would scarcely survive the night. She gradually began to improve, and after a slow convalescence regained her health. I have never seen a patient make a recovery after having been so ill.

A case of rupture of the vagina, with extrusion of the fetus and placenta into the abdominal cavity, has come under my care, but cannot be considered here, as it is not a case of rupture of the uterus.

It has already been reported in *The American Journal of Obstetrics*, vol. xxxvii., No. 4, 1898.

Treatment. As I have already indicated, the treatment must be taken up according to the class of cases met with. In the first class of cases no treatment will avail. The patient is practically moribund before the physician in charge is able to call counsel around him. The hemorrhage and shock prove fatal in too short a time.

In the second class of cases, in which there are no symptoms to indicate that rupture of the uterus has occurred, in which it is late before the actual condition is discovered, the treatment must vary from that to be carried out in the next classes. The treatment in this second class must be that indicated in the cases reported—namely, thorough drainage and thorough cleansing from below. I cannot see that anything but evil can result under such circumstances from an abdominal operation. Adhesions of intestines that have already been formed to protect the general peritoneal cavity must be broken down, and the danger of a general infection as a consequence be increased. I feel, therefore, that the golden opportunity having slipped by, we must be satisfied to allow the septicemia to proceed and deal with the case on conservative lines. I have reported two cases that have recovered through this line of treatment.

In the third class of cases, in which the rupture is recognized, in which the patient is not moribund from shock and hemorrhage, there can be, to my mind, only one line of procedure—namely, a thorough inspection of the part through an abdominal opening, a removal of blood-clot from among the intestines, a thorough stoppage of hemorrhage from the wound either by approximating sutures or gauze packing, and the establishment of thorough vaginal and abdominal drainage. I have reported one such case with an easy recovery.

To my mind, suturing of the rent is scarcely called for. It prolongs the operation, and this is of great importance under the circumstances. Anyone who has had practical experience of this tragedy of the lying-in ward must have noticed that the edges of the wound are so bruised as scarcely to hold a suture. To pare them off means an increased hemorrhage, and, as a consequence, increased delay. I therefore prefer to pack with gauze, and am satisfied that the uterus afterward becomes perfectly normal. The doctor on whose patient I operated in this way told me that on subsequent

examination he found her pelvic organs in a perfectly normal condition. To perform a hysterectomy under such circumstances must prolong the operation, increase the shock and unsex the patient.

The treatment of the fourth class of cases must bring up for our consideration the question of diagnosis of uterine rupture and uterine puncture. How are we to learn how to discover this condition when it exists without the presentation of any early symptoms? When the symptoms of inflammation and septicemia set in it is too late for us to make a diagnosis. I believe it would be wise, in one class of cases I have mentioned above, in which the pulse is abnormally rapid when compared with the slight difficulty of the labor, to explore the cavity of the uterus thoroughly with the finger, in order to ascertain the presence of any rupture. I have so frequently removed the placenta from the interior of the uterus in six, seven and eight days after delivery, and where neither hemorrhage nor odorous discharge indicated its presence, after the woman has become profoundly septic, that I differ in my opinions from the ordinary teachings of the text-books. I do not look upon the interior of the uterus as a sort of sacred inner chamber into which the finger of the accoucheur should seldom enter.

Whenever I am called to see a case of septicemia following labor I insist on making a thorough exploration of the interior of the uterus with the finger, either with or without the use of an anesthetic. I feel satisfied that retained placenta is a very frequent cause of such septicemia, and that no living accoucheur can feel and see a placenta and say that it is beyond a doubt intact after it has been expressed from the uterus. If, then, the thoroughly sterilized finger of the accoucheur is forced to hunt about in the uterus for retained placenta on the seventh, eighth or ninth day after labor, why should not this search be instituted earlier?

I do not believe that, given a vagina free from the gonococcus and a thoroughly disinfected finger, there is any danger to the woman from the introduction of the finger into the uterine cavity. Holding these views, I feel, therefore, that when the faintest suspicion of rupture of the uterus exists, a thorough exploration of the interior of the organ should be carried out, in order that the patient may have the benefit of early treatment.

A few years ago the uterine sound was looked upon as an instrument likely to be followed by bad symptoms. When we discovered

that it was not the uterine sound that did the damage, but the disturbance of adherent pus-containing tubes, we learned how to avoid the danger. So it is with exploration of the uterus after delivery of the after-birth. We can learn how to avoid the danger. There is no reason why the handling with a clean finger of the placental site should be any more dangerous than the handling of any other wound. I have explored the interior of the uterus a great many times and have never met with any untoward results, except in one case in which gonorrhea was present and I was not aware of the fact.

In a case I have previously reported of rupture of the uterus by the hand of the practitioner, the rupture would not have been discovered had not the attendant been suspicious that something happened. He instituted an examination and found intestine in the uterine cavity.

The portion of the treatment for rallying the patient has not been touched upon. You are all familiar with the usual routine, consisting of the intravenous saline solution, submammary saline solution, direct transfusion. In the first class of cases no such treatment will avail; death occurs too soon. In the class of cases that have recovered, such as those that I have recorded, this treatment was not found necessary.

And, finally, I may say that there is one point firmly fixed in my mind—namely, that when rupture of the uterus has been discovered within twenty-four hours after its occurrence, and the patient is not moribund, the abdomen should be opened, the abdominal cavity should be thoroughly cleansed, hemorrhage should be checked and drainage instituted. The operation must, of course, be carried out with every aseptic detail. I am satisfied that such treatment will give the best results.

DISCUSSION.

Dr. W. Japp Sinclair, of Manchester, England.—Mr. President: I appreciate the honor of being called upon to say a few words on this subject. I have not seen very many cases of rupture of the uterus, and my own experience is that treatment has not been very successful. So far as I know, I have never ruptured the uterus myself,

but I have had a few cases in which this has occurred in the hands of other practitioners; and I must say, without exception, it has been the result of sheer ignorance and want of judgment. I recall one case in which I was asked to see the patient by a colleague who had exhausted himself by pulling a pair of forceps, with two strong women holding the patient from being dragged off the bed, trying to deliver the child. I saw that the head had presented a little, but for some reason or other it was very difficult to deliver the child, and the patient changed a good deal for the worse, and so this fellow-practitioner conferred upon me the distinction of helping him out. I made a careful examination and found that the fetus was in the abdominal cavity, and I had to perform an abdominal section. The uterus was extensively ruptured, and in drawing down the anterior cervix posteriorly I did the only thing that seemed feasible—that is, I performed a Porro operation. The patient had been in labor for two or three days and was in the hands of a nurse. My colleague had made several attacks on her, and the result was she died in a few hours after I had had my opportunity of operating.

I recall another case in which the patient died just after the delivery of the child, the uterus having been ruptured in a similar manner, owing to want of judgment. In this case the midwife had had her turn, and had given a large quantity of ergot of rye, shortly after which she sent for a general practitioner. He sent for a colleague of mine, and my colleague in turn sent for another physician, and then they gave me a chance. When I arrived the fetal head was above the pubes. The patient was under the influence of an anesthetic, and the attending practitioner was perspiring profusely. He told me that he had pulled as he had never pulled before; he was very eloquent on his pulling at the beginning of our consultation, but never said a word about pulling after I had made an examination of the patient. I pushed my hand into the abdominal cavity, and the only thing I proceeded to do was to get the head down, perforate it, and drag it out. What else could be done under the circumstances? The child was removed in that way. A curious thing was the condition of the uterus. On making an abdominal palpation, after removing the child, the uterus gave me the impression of a felt hat when you make pressure upon it. The patient was very low. There was little or no hemorrhage. It was deemed best to let the patient alone for a few hours, with the intention, if she recovered sufficiently, of removing the uterus. But she did not rally, and died on the same day. Those were the worst and most striking cases I have encountered in the course of twenty-five years' practice. These cases occurred many years ago,

when the only thing to be done was to remove the uterus. Since then I have seen a few cases of rupture of the uterus. In most of them I have not found occasion to remove the uterus or to stitch it; the simplest process is to open Douglas's space, insert a drainage-tube, and trust to surgical procedure afterward. There have been no deaths since that method has been adopted. My impression is, with reference to those cases where the intestines come down, that if you put the patient under an anesthetic, push the intestines back, and give the uterus a chance to contract, with efficient drainage, you give the patient the best chance for her life.

Dr. Willis G. Macdonald, of Albany, N. Y.—With reference to rupture of the uterus, I recall a case in which the child was delivered, and the placenta and a number of feet of intestine presented to the outside world, which the practitioner cut off. He must have cut off fully five feet of the intestines. Six hours later I was called, and made an anastomosis with the Murphy button, but there was such a mixture of feces and blood in the abdomen that the patient died.

Dr. Ross (closing the discussion).—There was one point brought out by Dr. Sinclair which is of importance—namely, that when the child is extruded into the abdomen it is not wise to interfere from below, but it is much better to have the patient removed to a hospital, prepare her for a celiotomy, and do everything from above. In one case I had the child was extruded from the posterior vaginal wall, and the placenta was up under the liver. Delivery was very difficult to accomplish, and the child was turned inside the abdominal cavity before I saw her; the meconium was distributed among the intestines more than it otherwise would have been. After the child has once escaped into the abdomen, it is better to leave everything below and make an opening from above, and thoroughly clean out.

Regarding the case spoken of by Dr. Macdonald, in which a practitioner removed a number of feet of intestine, I would like to say that in one case I recall the fetus was extruded into the abdomen and the rectum was down in the vagina, but with a fair amount of tactile sensibility in my fingers, I could not determine whether it was placenta or bowel; it had become congested and thickened, the appendices epiploicæ rapidly enlarged, and the woman was fairly fat, so that it was impossible to say whether it was intestine or placenta. I soon determined the question by inspection.

RETROPERITONEAL TUMORS.

With Report of a Case and Presentation of Specimen.

By RUFUS B. HALL, M.D.,
CINCINNATI.

The etiology, pathology, and diagnosis of retroperitoneal tumors were so thoroughly discussed two years ago by one of the Fellows of this Association before the Southern Surgical and Gynecological Association at their meeting in St. Louis, that nothing need be said concerning these portions of the subject. To avoid repetition I shall omit all reference to them and present a case-report with a specimen.

Retroperitoneal tumors that are operable are rare. My experience in dealing with them, so far as removal of the tumor is concerned, has been limited. All retroperitoneal tumors are difficult to remove, and the operation is attended with a high mortality They are often malignant. The specimen presented to-day is probably a fibroid tumor of the uterus. When the tumor was removed eighteen months ago it was so soft and so unlike a fibroid tumor in appearance and consistence that I doubted this very much. The attachment to the uterus was so indistinct that one would doubt that it had its origin from that organ. You will observe from the specimen that the tumor was wholly post-peritoneal. It occupied the patient's right side, almost wholly filled the pelvic cavity, and projected well into the abdomen.

Mrs. B., aged thirty-eight years, consulted me February 14, 1898. She was well nourished and always enjoyed good health until eight months before her visit to me, when she first noticed a small tumor in the right iliac region, not larger than a small orange. The patient, being a very intelligent woman, gave a clear and

precise clinical history up to the time of her visit, which was corroborated by her family physician, Dr. Rowe, of Cincinnati. She had been married seventeen years and was the mother of two children, the younger thirteen years old. She had never had a miscarriage or even been treated for any pelvic or uterine disease previous to the present illness. She did not suffer much inconvenience other than "a heavy, dragging feeling" in the pelvis and abdomen.

Upon physical examination we found a tumor that had the appearance of a semisolid mass, nearly as large as an adult head, occupying the right side of the abdomen and extending well down into the pelvic cavity. The uterus, which was not enlarged, was retroverted, and the cervix presented at the vulva. The patient complained of frequent micturition, which was without pain. From physical examination one was inclined to believe that the tumor was solid, probably a fibroid tumor of the uterus fixed by adhesions; yet the clinical history of rapid growth did not coincide with this conclusion. The possibility of an intraligamentous ovarian tumor was discussed, but could not be certainly determined. The patient was apprised of the fact that the operation for the removal of the tumor would probably mean a hysterectomy. If on opening the abdomen the tumor proved to be an intraligamentous cyst, a hysterectomy might be avoided.

She entered my private hospital on March 7, 1898. The operation was made on March 10th, and the tumor here presented removed. Upon opening the abdomen I was quite surprised to find this large tumor entirely post-peritoneal. Near the centre of the tumor was the head of the colon, and the mesentery pulled up over the side of the tumor corresponded with the middle line of the patient's body. The veins and arteries over this surface were greatly enlarged, and to make an attempt to remove it looked like a hazardous surgical procedure, yet to leave it promised nothing. By taking hold of the tumor mass with the two hands it was slightly movable, suggesting that it might be detached without great difficulty. The dilated ureter could be seen over the top of the tumor to the right of the median line of the tumor. The peritoneum could be picked up between the fingers over the top of the tumor. I regarded the tumor at that time as a fibroid tumor of the uterus developing from the uterine body near the junction of

Retroperitoneal tumor.

the cervix with the body, and at once decided to make a hysterectomy with removal of the tumor. The operation was made in the following manner: I first ligated the ovarian artery on the patient's right and divided the vessel between two ligatures. I then ligated the ovarian artery on the opposite side, divided it between two ligatures, cut through the broad ligament on that side, and divided the peritoneum above the top of the bladder over to the tumor, and pushed the bladder down. I then ligated the uterine artery on the patient's left side, cut across the cervix, and clamped the uterine artery on the patient's right. I then divided the peritoneum behind the tumor up to a point on the tumor connecting with the incision made in front of the uterus. I then enucleated the tumor from below upward without much difficulty and with little loss of blood. There was general oozing from the large raw surface, and this induced me to make a total extirpation of the cervix and drain through the vagina. My greatest anxiety during the operation was to avoid injuring the ureter. There was no injury to this important structure. The patient made an uninterrupted recovery, leaving the hospital inside of five weeks, and remains in good health at this time.

I have had microscopical slides prepared from a section of this tumor, and have placed them under the microscope here, where the Fellows may see them. There is a difference of opinion among the experts as to the nature of this tumor. Some say that it is a spindle-celled sarcoma, and others that it is not malignant at all. At the time of its removal I felt reasonably certain that it was malignant. The clinical history favors this opinion, yet the appearance of the tumor now is that of a fibroid of the uterus. It is too early yet to say definitely from the clinical history of the patient whether it is malignant or not. I know of one patient who was operated upon by a friend of mine for a sarcoma of the left ovary. She made a perfect recovery, lived ten years, and I made a second operation a year or more ago and removed the opposite ovary for a perfectly smooth, round, pearly-white, hard tumor that proved to be sarcoma. The patient recovered and is now enjoying good health.

The record of this case shows that it is not always necessary to abandon an operation when the tumor is found to be retroperitoneal. In some cases these tumors can be removed without great

difficulty, and the patient will make a complete and lasting recovery. It may not always be possible to determine which case could be safely operated upon until after the abdomen is open. If the tumor is somewhat movable and the peritoneum not firmly agglutinated to the tumor mass, these tumors can be removed. Sufficient time has not elapsed since my other cases were operated upon to justify reporting them as permanent recoveries.

DISCUSSION.

Dr. Charles A. L. Reed, of Cincinnati, Ohio.—There is but little to discuss about this case, except the one question of operative procedure, and after mature deliberation there can be but one opinion in regard to that. The technique of the operator in this case was very similar to that with which his name has become identified during the last two years. The technique, while somewhat a modification of the principle of hysterectomy adopted by Kelly and others, is still original with the reporter of this case as applied to intraligamentary and retroperitoneal tumors in this situation. Of course, the question stands out as to the expediency of doing hysterectomy in a case of intraligamentary tumor upon one side. I relaxed my attention somewhat during the reading of the doctor's paper and did not catch whether the appendages were diseased on the other side or not.

Dr. Hall.—They were perfectly normal.

Dr. Reed (resuming).—That illustrates a point that may be raised in the minds of some operators. I have had occasion to deal with a few intraligamentary growths, and they are not essentially different from cysts, so far as the anatomical conditions to be dealt with are concerned. It makes but little difference whether the tumor is solid in this situation or cystic so far as the difficulty of removing it is concerned. Its removal by the old technique—namely, that of direct enucleation by splitting the peritoneum and pulling it out—is a procedure that is fraught with so much danger, primarily and secondarily, that it has occurred to me that this modified technique is certainly justifiable, although it carries with it the ablation of the appendages of the other side and the practical termination of the woman's reproductive life. I do not know to what extent that observation may apply with reference to the age of this patient.

Dr. Hall.—She was thirty-eight.

Dr. Reed (resuming).—Therefore, I think that this procedure was justifiable. It is certainly a very ingenious method of dealing with

these cases, beginning the enucleation upon the other side of the pelvis, securing hemostasis in advance, dividing the bloodvessels, making a hysterectomy, and then peeling out the tumor from below rather than from above. Some of the most serious cases of this character have been those in which the sequela of the operation has been remarkable, chiefly on account of damage to the ureter. I recall one case that occurred in my practice a good many years ago in which I had occasion to remove a very large tumor which, while it was evidently retroperitoneal in its origin, developed until it was simply keeping close to the peritoneum. It had lifted everything above it. I divided the ureter, resorted to an anastomosis, but it failed, and finally I had to do a nephrectomy to secure the comfort of the patient; and while she is living and well today, still that is not a desirable course for these cases. With this modified technique we can more easily avoid a complication of this character, and I think it is one well worthy of the serious consideration of operators in this department.

DR. WILLIS G. MACDONALD, of Albany, N. Y.—I would like to inquire of Dr. Hall the relation of the common iliac artery and vein to this growth?

DR. HALL.—They were behind it, but the ureter was lifted up in front of the tumor.

DR. MACDONALD (resuming).—It seems to me that in the investigation of the character of retroperitoneal tumors differences of opinion among pathologists may very reasonably occur; but the clinical history, either before or subsequently, shows that by far the great majority of these tumors have a sarcomatous element in them. While they may be in the nature of a fibroma, a fibromyxoma, or a fibrolipoma, yet in certain areas of growth there will be more or less evidence of spindle-celled sarcoma. A few months since I adopted a somewhat different technique in the removal of a growth which weighed a little more than seven pounds, having a similar origin to the one mentioned by Dr. Hall. After making a very short incision and determining the fact that it was a retroperitoneal tumor, passing ever close to the wall of the pelvis, not near the origin of the right kidney, I simply closed the short incision which I had made, rather less than two inches, placed the patient in the lateral prone position, and commenced an incision from the apex of the twelfth rib, carried it down and forward to a point perhaps an inch (or a little more) internal to the anterior spine of the crest of the ilium, and thence obliquely forward along the outer border of the rectus muscle to Poupart's ligament, and we found almost identically the same relations of the growth—that is, the ureter which no one could miss. In looking at

it, we stripped up the peritoneum, and as we came upon the ureter there was very little bleeding. We could readily trace it in the groove running over the anterior and internal surface of the tumor. It separated very readily indeed. The tumor lay in contact with the lower border of the kidney. Its enucleation was not attended with any great difficulty. We ligated some large vessels in the pelvis; we did not open the second time the peritoneal cavity, and simply left in a small wick drain. If one can be satisfied with the relations of the growth, it seems to me that this is a more preferable incision. A great deal can be done, however, through this oblique lateral incision in relation to a good many organs. It is practical. You begin the incision described in the text-books for the ligation of the common iliac, extend it upward and downward a little, and it certainly has astonished me very much as to the amount of surgical work one can do, and the closeness with which one can get to the abdominal aorta without having any serous hemorrhage or serious shock following operation. I believe that in retroperitoneal growths this incision is preferable, for the reason that in the dissection and stripping up the peritoneum we do not interfere with the circulation either of the cecum or on the left side with the descending colon and the sigmoid.

Dr. Hall (closing the discussion).—I take this opportunity, in view of the operation narrated, to say that I do not think any operator would hesitate a moment as to the advisability of doing a hysterectomy for cutting off the blood-supply before attempting the enucleation of this tumor. The tumor could only be completely removed by rolling it over on itself and lifting it out of the pelvis. So far as the safety of the ureter is concerned, it looked as if the ureter was in a state of high tension at the time of the operation, and that in enucleating the tumor and sweeping around one could not very well avoid injuring the ureter. The blood would run over the place at once; there would be great loss of blood. This woman was thirty-eight years of age, the mother of two children, and anxious to have more. She had been sterile eleven years, and would probably have remained so provided she did have an operation. There was some question as to whether it was malignant or not, and all such things were discussed with the patient. I told her that I would take out the tumor, if possible, and I thought it was possible. At first I thought it was a fibroid; as I proceeded with the operation I found it was not, but I went on with the operation, cut off the blood-supply first by sacrificing the uterus and ovaries, and I believe any other method of procedure in this particular case would have sacrificed the patient's life from hemorrhage. When it comes to dealing with intraligamentary cysts, I think the same rule should

govern our procedures. If we are going to sacrifice a patient's life from hemorrhage by enucleating the tumor from above, with the hope of saving the uterus and ovary, we had better save life by sacrificing the uterus and ovary. As operators, we must do our surgical duty in the best interests of the patient. The last word has not been said with reference to operations for intraligamentous cysts.

As to the question of recurrence in intra-abdominal cancer or sarcoma, the patient referred to in the paper was operated upon a year or more ago. She was a young girl, sixteen years of age, when operated upon the first time, and the sarcoma was removed. She is the cousin of a prominent physician; he was present at the operation. Many years have been added to her life. She may have a recurrence, and probably will, but she was anxious for another respite. She knew she had another tumor, and she was anxious to have an operation performed a second time. This being the case, it is our duty to operate in such instances.

I wish to congratulate the last speaker upon the successful termination of the operation referred to, on the ground that in many cases that would be the preferable operation.

WOUNDS OF THE LIVER AND BILIARY TRACT.

By W. E. B. DAVIS, M.D.,
BIRMINGHAM.

PENETRATING wounds of the liver are not common. Slight injuries as a rule recover; severe ones prove fatal from hemorrhage if not promptly cared for. Injury to the biliary canals and the extravasation of bile may contribute to the fatal issue. These wounds are most frequently caused by the surgeon in operating for hydatid cysts and abscesses. I report the following cases, which occurred within a few weeks of one another:

CASE I.—Capt. W., aged sixty-two years, quite fleshy, a great brain worker; had suffered from dyspepsia for years. He was seized with violent pain over the region of the gall-bladder, and required a hypodermatic of morphine, ¼ grain, before being relieved. The liver was tender and extended an inch and a half below the margin of the ribs. The skin was slightly discolored. There was nausea and some vomiting; temperature was 102° on afternoon of attack; on the next day 99.5° at morning and 100.5° in the afternoon; pulse less than 100. The daily temperature remained the same for a week, everything pointing to an abscess of the liver. Patient was seen by Dr. Holmes, of Atlanta, who happened to be my guest at the time, and by my brother, Dr. J. D. S. Davis. On October 28, 1898, one week after attack, the abdomen was opened and the right lobe of the liver aspirated, not more than half an ounce of pus being removed. I withdrew the needle, thinking there was more pus, but was mistaken. Made incision, and with the finger searched for this small cavity, and in doing so produced a wound of considerable depth and great size—perhaps two inches in length. There was very profuse hemorrhage, which was controlled by iodoform gauze packing. The liver tissue gave way under the packing, making the wound much larger. Patient stood operation well. He

suffered from nausea for a couple of days, which was relieved after the bowels acted freely. A large quantity of bile was discharged through the gauze, necessitating frequent change of dressings. The bile continued to pass in large quantity for three weeks, and in small quantity another two weeks, about the amount usually discharged in a case of cholecystostomy. The liver rapidly decreased in size, and the patient is in a better condition now than for several years. The extensive wound of the liver evidently assisted in his recovery by severing the small biliary canals and emptying the liver. The pain at the onset of the attack was probably due to the rupture of one or more of these canals with the extravasation of bile in the substance of the liver.

CASE II.—Mrs. S., of Shelby County, aged twenty-eight years, consulted me in December, 1898. She had suffered from attacks of epigastric pain and fever for four months. There was some tenderness elicited by pressure over the liver, the margin of which extended to midway between the ribs and umbilicus. There was no enlargement of the gall-bladder. Several attacks of pain would occur in a day or a week, coupled with slight jaundice. There would be nausea and some vomiting. The chills and fever simulated malaria. The diagnosis of stone loose in the common duct was made. Operation was advised as soon as present attack was over. In a few days, however, she was seized with violent pain in the right hypochondriac region, with fast pulse, 130 to 140, temperature 103°, and vomiting. The peritonitis was localized. It was decided to make a drainage operation to bridge the patient over, and subsequently an operation to remove the stone. At our private hospital, in the presence of Drs. Copeland and Morris, of Birmingham, the abdomen was opened, December 27. There were extensive adhesions to the under surface of the liver, which were partly separated. There was no pus around the liver, but great bleeding occurred, which was controlled by gauze packing. The right lobe of the liver was aspirated, and it was thought that some pus was removed, but a more thorough examination showed that this was incorrect. Made free incision in right lobe, which was followed by considerable bleeding. Iodoform gauze was packed in the liver wound. There was considerable discharge for some days of a dark bloody fluid; not a great deal of bile. The liver returned to its normal size in a few weeks, but there were occasional recurrences of epigastric and sub-

scapular pain, with fever, showing that the stone was still in the duct. The spleen became very much enlarged four months after the operation. About two months later, when the patient had decided to have the radical operation for the removal of the stone, it was passed. I saw her recently, and she was looking exceedingly well.

CASE III.—Mrs. H., of St. Clair County, was referred to me by Dr. J. W. Ash, of Springville, Ala., December, 1898, aged sixty years. Six weeks previously without great pain she rapidly became jaundiced. The jaundice gradually became more intense. Liver extended almost to umbilicus. There were no nodules to be made out. The patient's condition was extremely unpromising, and no encouragement was given the family. An exploratory operation was done December 31. The gall-bladder was empty and quite white. No stone could be made out in the common duct. A thorough investigation of the under surface of the liver was not made, owing to the very critical condition of the patient and the size of the liver. However, it was evident that the obstruction was in the hepatic duct or its branches. The bladder was attached to the abdominal incision, more to anchor the liver than otherwise. An incision was made in the right lobe, and the bleeding was very free, but controlled by iodoform gauze packing. Patient lived five days and died from exhaustion. I had hoped to sever some of the biliary canals, and thus drain the liver till patient was somewhat relieved of cholemia. The autopsy revealed a large malignant nodule in the transverse fissure which completely obstructed the branches of the hepatic duct. The biliary canals throughout the liver were very much distended.

Wounds of the biliary tract beyond the liver produce death, as has been well said, "not so much from the sudden escape of bile as from the continuous pouring of fresh bile into the peritoneal sac." Quarts of fluid may not produce death if there is time for adhesions to form and protect the general cavity from the fluid.

Four years ago Dr. Stuart, of Attala, Alabama, consulted me in regard to a case of gunshot wound in the region of the gall-bladder. A large collection had occurred in the right side of the abdomen. The feces became clay-colored, showing the absence of bile from the intestinal tract. By repeated tappings by Dr. S. the patient was cured. Large quantities of bile were removed at the first operation.

Symptoms depend upon the complications and upon the protective adhesions. If there are protective adhesions there will be symptoms only of localized peritonitis. Otherwise death will generally quickly result. Cases have been reported where the patient did well for a few days, and then became very ill and soon died. Evidently this was caused by the giving way of the adhesions.

In two cases where I operated on the biliary tract in animals with gauze drainage the abdominal incision became closed, and the animals died from this accident.

Rupture of the gall-bladder and the biliary ducts, as well as the biliary canals in the substance of the liver, may occur from external violence, such as a blow. The gall-bladder ruptures more readily when filled with stones. The liver may be completely torn through, or the rupture may be limited to the upper or lower surface.[1] Ogston collected 26 cases in which the lesion was in the right lobe in 9 cases; in the left lobe in 11 cases; in the central parts of the liver in 6 cases. The hepatic veins generally escape injury in these lacerations. "There may be a complete division of the liver, the parts being held together by the veins." It has been shown by a number of observers that these cases suffer from extreme pallor, coldness of the surface, slow, feeble pulse, difficult breathing, distention of the abdomen, great pain, thirst, nausea and vomiting. If patients live more than twenty-four hours they usually suffer from itching of the skin and jaundice; later, according to Bernard, from sugar in the urine. Death may result within a few hours from hemorrhage or shock; within a few days from toxemia and peritonitis.[2]

In 5 cases in which I operated on dogs I removed more than one-third of the liver in 4 cases. All of these died from shock and peritonitis in from one to two days. In the other case the extreme left lobe of the liver was removed and the animal made a complete recovery. Evidently in extensive removals of liver tissue the mortality is large, while small portions of the liver may be removed with comparative safety.

Case I.—August 21, 1899. Two lobes on the right side of the liver and the gall-bladder were removed; stump ligated with large silk; dog died in twenty-four hours; very small quantity of blood in cavity; slight redness of intestines; fluid in cavity bile-stained.

[1] Ashhurst's System of Surgery. [2] Ibid.

CASE II.—August 21, 1899. Same operation as Case I. Dog died on August 22. Small amount of bloody fluid in peritoneal cavity.

CASE III.—August 22, 1899. Two lobes on left of liver removed; gall-bladder not disturbed; dog died the next day. Autopsy showed very small quantity of bloody fluid in cavity.

CASE IV.—August 22, 1899. Entire left lobe of liver removed; Lobe on right somewhat injured by manipulation; dog made uninterrupted recovery. September 2, dog was killed for examination; no peritonitis; left side of liver firmly attached to stomach.

CASE V.—August 26, 1899. Two lobes of liver and gall-bladder removed; gauze drain used; died 28th. Autopsy, some bile and blood in cavity; slight peritonitis.

The following experiments demonstrate that small quantities of bile may be injected into the peritoneal cavity without harm:

CASE I.—August 14, 1899. Incision in median line; contents of gall-bladder of another dog (not more than a dram) injected into cavity; dog was not made sick by operation. September 2, 1899, dog killed; nothing found.

CASE II.—August 21, 1899. Incision in median line; small opening made in peritoneum, and five drams of bile, removed from two other dogs in which the common duct had been ligated, injected into peritoneal cavity; incision closed; used iodoform and collodion dressing; dog gave no evidence of being sick. September 2, dog killed; nothing found.

CASE III.—August 14, 1899. Incision in median line; cystic duct ligated; gall-bladder incised and the bile allowed to flow into peritoneal cavity; bile from another dog also injected into cavity. September 2, 1899, dog killed; some adhesions; no peritonitis; gall-bladder opening had closed; there was a hard mass of inspissated bile in bladder, producing a well-formed gallstone.

CASE IV.—August 15, 1899. Incision in median line; piece of gauze saturated with bile from another dog placed in cavity just beneath liver; dog killed September 2, 1899. Adhesions, gauze walled off thoroughly; abscess around gauze.

CASE V.—August 16, 1899. About one-half dram of bile, with equal quantity of sterile water, injected subcutaneously. August 26, 1899, five drams of bile injected into cavity. This bile was taken from a distended gall-bladder produced by ligation of the com-

mon duct; dog made uninterrupted recovery. September 2, animal killed; nothing found.

The following experiments, 23 in number, show the effect of bile in wounds of the biliary tract. It will be observed that where there was a small escape of bile the animals recovered, and where there was considerable extravasation they died in from one to three days. The abdominal viscera would be highly bile-stained, but there was not the redness of the intestines that is observed in septic peritonitis. Death was evidently produced largely by toxemia. These cases are taken from two series of experiments, one in 1895 and the other in the present year.[1] They do not include those made in 1892, at which time I conducted experiments to demonstrate that gauze-drainage would protect wounds of the liver and biliary ducts, and referred to in a paper read in that year before the Surgical Section of the American Medical Association:

CASE I.—August 19, 1885. Common duct incised and separated from duodenum; no drainage; dog died within twenty-four hours. Post-mortem, general peritonitis. Abdominal cavity was probably infected during operation.

CASE II.—August 19, 1895. Cholecystectomy without ligation of cystic duct; abdominal incision completely closed; no drainage; animal recovered. In cases where the cystic duct is not tied the discharge of bile into cavity is usually quite limited.

CASE III.—August 22, 1895. Piece of gall-bladder the size of a dime removed; no drainage. On August 24, strangulated hernia was found, due to the stitches giving way; bile was found in the peritoneal cavity; no peritonitis to speak of; dog died from shock after second operation.

CASE IV.—August 24, 1895. Piece of gall-bladder removed; no drainage; died in twenty-four hours; no post-mortem.

CASE V.—August 26, 1895. The common and cystic ducts were incised and the gall-bladder detached from the liver; no drainage; dog lived twenty-four hours.

CASE VI.—August 29, 1895. Piece removed from gall-bladder; abdominal incision was closed; dog opened twenty-four hours after

[1] Both series of experiments were conducted at the Birmingham Medical College. In those of 1895 I was assisted by Dr. Geo. A. Hogan, of this city. This year Dr. Edgar A. Jones, Lecturer at the Birmingham Medical College, and Messrs. Robt. E. Hogan and Archer Sims, medical students, assisted me. Dr. Jones made the autopsies.

operation, and had general peritonitis; cavity was infected at time of operation.

CASE VII.—August 29, 1895. Piece removed from gall-bladder and abdominal incision closed; no drainage; opened August 30; great quantity of bile in cavity, and some peritonitis; drainage was then introduced, but the dog died from shock.

CASE VIII.—August 29, 1895. Gall-bladder removed, and cystic duct opened down to the common duct. August 30, animal re-opened; some peritonitis.

CASE IX.—August 30, 1895. Gall-bladder amputated and gauze drain introduced at lower part of abdominal incision; animal died from shock.

CASE X.—August 30, 1895. Portion of gall-bladder removed; bile poured into lower part of abdominal incision; dog died about twenty-four hours afterward; no post-mortem.

CASE XI.—July 31, 1899. Gall-bladder incised—small opening—and bile removed; no closure of gall-bladder. August 10, 1899, dog killed; some adhesions around gall-bladder; no peritonitis.

CASE XII.—July 31, 1899. Gall-bladder incised; bile removed; no closure of gall-bladder. August 10, 1899, dog killed; gall-bladder region walled off.

CASE XIII.—July 31, 1899. Gall-bladder dissected away from liver and removed; no ligation of duct. Second operation August 3, 1899, original wound dry; second incision made in median line; nothing done in the abdominal cavity. August 10, 1899, killed with chloroform; adhesions in region of gall-bladder; no bile in cavity; intestines slightly discolored.

CASE XIV.—July 31, 1899. Gall-bladder incised freely and bile allowed to flow into peritoneal cavity. Died August 1; some adhesions; bile in cavity; no peritonitis.

CASE XV.—August 1, 1899. Gall-bladder detached from liver; common duct slit open; death occurred a few hours after the operation; no peritonitis.

CASE XVI.—August 1, 1899. Gall-bladder excised; cystic duct slit down to common duct and removed and common duct opened; dog died in twenty-four hours; no autopsy.

CASE XVII.—August 1, 1899. Cystic and common ducts incised; considerable manipulation; intestines handled a good deal; died August 3, 1899; large quantity of bile-colored fluid in abdom-

inal cavity; slight adhesions; intestines and omentum very much discolored with bile.

CASE XVIII.—August 5, 1899. Common duct incised near duodenum; bile escaped freely into general cavity; dog died in forty-eight hours; no autopsy.

CASE XIX.—August 14, 1899. Gall-bladder incised; bile escaped into cavity; autopsy, August 15, 1899. Peritoneal cavity filled with bile; omentum, intestines, stomach, and liver very much bile stained; gall-bladder empty; some redness of intestines, also portion of pyloric end of stomach; intestines considerably distended with gas.

CASE XX.—August 14, 1899. Gall-bladder incised; cystic duct slit up its entire length; dog died in forty-eight hours; no autopsy.

CASE XXI.—August 15, 1899. Gall-bladder very freely incised and bile caught on gauze sponge; dog died August 17, 1899; no peritonitis; everything bile stained; bile in cavity; some adhesions.

CASE XXII.—August 29, 1899. Common duct opened; killed September 2; all viscera very much bile stained; omentum intensely so; mesentery less; duct open; bladder distended; very numerous and strong adhesions; not much bile in general cavity; large quantity of bile in and around gall-bladder, the gall-bladder region being walled off from general cavity.

CASE XXIII.—August 29, 1899. Gall-bladder and cystic duct incised throughout whole length of both; escaping bile caught on gauze sponge; dog killed September 1, 1899; omentum and mesentery very much discolored; little or no discoloration of intestines.

The treatment of these wounds consists in the prompt opening of the abdomen and controlling bleeding by stitches and gauze-packing. The gauze also protects the general cavity from bile. The experience of those who have dealt with these injuries demonstrates the correctness of this treatment. My experience shows conclusively that the abdominal cavity can be protected from bile in this way. I have produced all sorts of injuries of the liver and biliary tract and resorted to this treatment. The gauze was allowed to remain from three to four days. The opening in the bladder or ducts will close as quickly as the fistula of a cholecystostomy.

The fallacy of depending on the symptoms of hemorrhage, or on the evidence of perforation of the intestines, as shown by the gas test, in penetrating wounds of the abdomen is patent. Bile may

escape, without great hemorrhage, and produce death. Exploratory incision should be made in all penetrating wounds of the abdomen to determine the extent of the injury. The following experiments are appended:

CASE I.—August 10, 1895. Cholecystectomy; duct not ligated; drainage with gauze; August 22, animal doing well; August 31, found dead, owing to closure of fistula; case was not looked after.

CASE II.—August 31, 1895. Cholecystectomy without ligating cystic duct; drainage with gauze. September 17, dog was anesthetized and reopened. The field of operation was completely walled off, the wall consisted of stomach, omentum, duodenum, abdominal wall and liver.

CASE III.—August 16, 1895. Cystic and common ducts incised; drainage with gauze; reopened August 17; field of operation completely walled off.

CASE IV.—September 14, 1895. Cystic duct was incised and the common duct severed; gauze drainage; September 17, dog was dressed; field of operation walled off.

CASE V.—September 14, 1895. Incised the cystic and common ducts; drainage with gauze; September 17, abdomen reopened and the field of operation completely walled off.

CASE VI.—September 14, 1895. Cystic duct incised down to the common duct and removed with the gall-bladder; gauze drainage; result as in preceding case.

CASE VII.—September 14, 1895. Gall-bladder incised and the common duct tied; drainage; September 16, dog reopened and the field of operation was completely walled off; no peritonitis.

CASE VIII.—September 14, 1895. Removal of the gall-bladder and the cystic duct; gauze drainage; animal got along well.

CASE IX.—September 17, 1895. Cholecystectomy; drainage with gauze. September 21, dog doing well.

CASE X.—September 17, 1895. Common and cystic ducts incised. Gall-bladder detached from the liver; drainage with gauze; field of operation was completely walled off.

CASE XI.—August 3, 1899. Common duct ligated with large silk. August 5, abdomen reopened; liver hard; gall-bladder not enlarged; duct somewhat distended; resected a portion of duct and allowed bile to escape on gauze; gauze drainage; death in forty-

eight hours; autopsy, no bile in cavity; protective adhesions around liver; intestines slightly discolored.

CASE XII.—August 4, 1899. Common duct ligated with small silk. Second operation August 10, 1899, original incision opened; gall-bladder incised and bile allowed to escape; gauze was packed around field of operation for drainage. August 11, 1899, dog killed; general cavity well protected.

CASE XIII.—August 18, 1899. Common duct ligated and slit open freely on proximal side; all bile escaping caught on gauze; no closure of duct. Second operation August 25, considerable adhesion of omentum to abdominal wall, protecting duct; ligature removed from duct; gall-bladder excised; gauze drainage. Autopsy, August 26, 1899; no sign of peritonitis; some adhesions.

CASE XIV.—August 18, 1899. Incision in median line; common duct ligated with large silk. August 26, original incision opened; adhesions broken up; gall-bladder incised; ligature removed from duct; opening in common duct; gauze drainage; animal did well and was killed in two weeks.

PAST AND PRESENT SURGERY OF THE GALL-BLADDER AND BILE-DUCTS.

By WILLIAM H. MYERS, M.D.,
FORT WAYNE.

THERE is no gland in the body which discharges such multifarious duties as the liver, and none more often deranged.

It destroys colored blood-corpuscles.

It secretes bile.

It excretes carbonic acid.

It intercepts certain poisons.

It constructs glycogen.

The Greeks showed their wisdom in the belief that the physical basis of mental life was closely associated with the functions of the liver. We find traces of this in the modern writings of our novelists. I will quote George Sand: "It is certain that sharp pains in the liver produce symptoms in all those that are subject to them of profound sadness and a wish to die. Since my disease first appeared I have had happy years, and when it seized me again, although I was in the condition most favorable to love of life, I felt myself suddenly seized by a desire for eternal repose."

So I declare that many a dark ecclesiastical dogma about man and God has had its inspiration in a diseased liver. It has shaped religious creeds; it has thrown overboard costly spiritual freight gathered in a lifetime; it gave us the lamentations of Jeremiah, and the ghastly sermons of eternal damnation of Jonathan Edwards. It buried Servetus.

The excretory apparatus of the liver consists of the hepatic duct, the cystic duct, the gall-bladder, and the common bile ducts.

The gall-bladder may be looked upon as a diverticulum of the hepatic duct; it lies in a fossa underneath the liver, pear-shaped, and is capable under ordinary circumstances of containing about one ounce of fluid; but it undergoes great alternations in volume.

It is not my purpose in this discussion to make a curious

and busy search for every local ill and attribute each one to the liver, for if I did you would accuse me of a resolute prejudice resulting from a still more resolute credulity; for, when a man has firmly grasped the idea that the stomach, or the uterus, or the liver is the only true source of all disorders in the human system, he ceases to trouble himself with riddles of diagnosis.

We need only glance at any surgical text-books published in the last decade to recognize that hepatic surgery did not then exist; the pathological lesions were recognized and described, but no procedures suggested to relieve or prevent.

In 1793, Petitt recommended simple incision for enlargement of the gall-bladder, the sequelæ of biliary retention; this suggestion was condemned by Brumfield.

Delpech, in 1816, opposed surgical interference, as did Dr. Mason Good, in 1825.

Dubardin, in 1828, directed attention to the obliteration between the gall-bladder and the peritoneum by adhesions.

Liston said in 1840: "But surely no practitioner of the present day would think of making an extensive incision through the abdominal parietes."

In Chelius's *System of Surgery*, published in 1847, the subject is dismissed by a few observations upon wounds of the liver and gall-bladder.

In Cooper's *Surgical Dictionary*, edited by Lane, in 1861, under "Gall-bladder," we read: "The surgeon may be called upon to puncture the gall-bladder in cases of obstruction of the biliary ducts; the proper instrument for such an operation is a hydrocele trocar."

In 1846, Copeland wrote an article on "Concretions." Biliary concretions he defined, "Concretions formed in the bile passages, and occasioning in many instances more or less disturbance, with paroxysms of pain commonly referred to the right hypochondrium, reaching to the back, with increased suffering while passing into the alimentary canal, and often giving rise to sickness or vomiting and severe attacks of colic." No better definition was ever presented. Referring to the treatment, he quotes Petitt as contending for the propriety of making an early opening into such tumors, with the view of evacuating the calculi. "But," he says, "the incertitude of adhesions having been formed between the fundus and the abdominal parietes, must prevent every physician from directing the performance of the operation."

Biliary concretions in the gall-bladder were first noticed by Fallopius and Versalius. They were afterward more accurately described by Glisson, Hoffman, Morgagni, Boerhaave, von Swieten and Haller, and even the more recent researches of Heberden, Soemmering, Thenard and Bostock failed to suggest relief from surgical procedures. They discovered biliary concretions in the radicals of the hepatic ducts, in the cystic duct, in the gall-bladder, and in the common duct. They also knew of ulcerations of the bile passages establishing fistulas, for they have presented the records or the relics of at least seventy cases where the gallstones were discharged through openings in the abdominal parietes.

In the *Edinburgh Medical Journal* of 1857, Dr. Murchison published an article on "Gastrocolic Fistula," in which the following was his classification under five varieties:

1. Between the gall-bladder and stomach.

2. Duodenal, the most common variety. The gall-bladder contracts; adhesions to the duodenum and ulceration spreads from the interior of the former into the latter. Numerous cases are cited.

3. Fistulas between the gall-bladder and colon appear to be intermediate between the two first varieties.

4. Fistulas between the gall-bladder and external surface of the abdomen are not very rare.

5. Fistulas between gall-bladder and peritoneum. If the gall-bladder forms no adhesions to the surrounding parts perforations into the peritoneum followed by fatal peritonitis ensues.

Despite the pathological evidence already quoted, and the great work of Petitt, nothing new in the way of rational treatment was presented, as is proven by a perusal of the works of Spence, Hamilton, Ashhurst Holmes, Gross, and Agnew. These great surgeons made no advance, for the reason that they believed that traumatism was uniformly one of the causes of peritonitis. We know now that a cut in the peritoneum when there is no sepsis heals rapidly; that the severest trauma may be inflicted on the peritoneum and no peritonitis follow.

In answer to a letter addressed to Granville Bantock, containing the following question, "Do you regard peritonitis as a frequent cause of death after intraperitoneal operations?" I received the following reply: "I do not regard peritonitis as a frequent cause of death after intraperitoneal operations. When, however, a patient dies from peritonitis I think it may be safely called a case

of septicemia. This is most likely due to direct infection at the time of operation." In 1871 they had not grasped the idea of the "clean touch;" they had not then opened to themselves the realms of safe operative work. Professor William MacCormac states in 1879 that a largely attended and influential meeting was held at St. Thomas's Hospital for the discussion of antiseptic surgery, proving that it was then in the experimental stage.

Incision of the gall-bladder for the relief of distention or the removal of calculi was first done by Dr. Bobbs, of Indianapolis, in 1867, and Marion Sims and Kocher, in 1878, established the operation. Few discoveries are made by one alone, and rarely any discovery is in the true sense original; the discovery is the product of the time; the credit is usually accorded to the one who combines the greatest ability with the greatest earnestness and diligence, and here the claim of Dr. Marion Sims is entitled to a fair consideration. When Dr. Bobbs incised the gall-bladder, removed fifty calculi, closed the incision by suture, and his patient recovered, it was the beginning of an epoch. Bobbs, Sims, and Kocher almost simultaneously were directing their attention to surgical procedures entirely new. If you will read the history of discoveries this has not been unusual. Out of a multitude of instances of this kind I will cite Adams and Leverrier, in astronomy; Virchow and Hughes Bennett, in pathology; Priestley and Scheele, in chemistry. From this epoch, inaugurated by Dr. Bobbs, the surgery of the liver has made and is still making rapid progress. Cholecystotomy is now accepted as the treatment for the relief of distention or for the removal of calculi from the gall-bladder, cystic, or common duct. For much of our knowledge of the procedure we are indebted to the writings of Mr. Lawson Tait, Mr. Mayo Robson, and to an admirable paper read by Mr. Knowsly Thornton before the Medical Society of London in 1890. They have suggested the following indications for operating:

1. In frequently recurring biliary colic with or without enlargement of the gall-bladder.
2. In enlargement of the gall-bladder accompanied by great pain.
3. In empyema of the gall-bladder.
4. In abscess.
5. In fistulas, mucous, mucopurulent, or biliary.
6. In certain cases of disease of the gall-bladder, when no definite evidence exists indicating the presence of calculi.

7. Where cholecystitis is present.

8. In all obscure cases when inflammatory symptoms in the region of the gall-bladder resembling peritonitis are present.

I have selected for your consideration four of my own cases which show exceptional features and seem to illustrate some principles of treatment.

Mrs. C., aged thirty-four years, was admitted into the St. Joseph's Hospital with the following history. She described her sufferings as follows: Excruciating pain in the right hypochondriac region, lasting several hours, usually occurring after meals, followed by local tenderness for several days; not the slightest evidence of jaundice present at any time. On examination I found a tumor to the right of the umbilicus, the lower part resting upon the brim of the pelvis, shape pyriform; it was free from tenderness.

The diagnosis was calculi in the gall-bladder, with obstruction of the cystic duct.

After being etherized an incision was made parallel with the fibers of the rectus muscle and beginning opposite the cartilage of the eleventh rib. I was enabled to bring up the fundus of the gall-bladder. It was lodged in the wound and held by a pair of pressure forceps. Gauze was then packed around the gall-bladder, and it was emptied by an incision, and manipulated to prevent any fluid from escaping into the wound or even into the gauze. Six calculi were removed. The stitching of the gall-bladder was by a continuous suture to the muscular tissue, after the insertion of a drainage-tube. This was allowed to remain fourteen days.

She was free from pain; all the symptoms which previously distressed her had disappeared; but a fistulous opening remained and continued to discharge mucus during the time she was under my observation.

In all cases of obstruction of the cystic duct we will find an enlarged gall-bladder, and we should resort to cholecystectomy or adopt the method devised by Dr. Murphy, of Chicago, of connecting the gall-bladder with the duodenum.

Mrs. B., aged twenty-three years, was admitted into St. Joseph's Hospital, February 1, 1897, with the following history: For the previous eighteen months she had suffered severe abdominal pain, accompanied by vomiting, lasting from twelve to twenty-four hours.

An examination could detect no tumor, only tenderness existed in the region of the gall-bladder. My diagnosis was gallstones, and this opinion was agreed to by Dr. Porter.

Under ether an incision three inches in length was made, commencing at the level of the tenth rib. The details of the operation were carried out on the lines usually adopted. The gall-bladder was found thickened and adherent to the under surface of the liver, containing numerous calculi of extremely minute size. A drainage-tube was inserted during the first three or four days after the operation. Mucus was freely discharged, followed by pure bile. She left the hospital February 27, 1897. The temperature never reached 100°. She was dismissed free from pain, much improved in color, and gaining in weight. For some time previous to the operation she had been jaundiced. This disappeared rapidly after the drainage had been established.

The third case which I will report is that of Mrs. H., aged fifty-six years. She had suffered many years with recurrence of biliary colic, sometimes every week, again every month, but had never passed over a year without an attack. Pains in shoulder and back, constipation, vomiting quite often, never bile. No tumor could be detected at the time of the admission to the hospital, March 4, 1898. Operated upon March 7th under ether. The usual incision was made, and the gall-bladder found with great difficulty. There were numerous adhesions with the transverse colon and the omentum ; the gall-bladder was shrunken and its walls thickened ; it could not be brought near the surface. It was carefully walled off from the surrounding tissues with iodoform gauze and an incision made. Slight mucous discharge followed ; the calculi were removed with great difficulty, as the gall-bladder had shrunken so as to embrace the calculi very firmly. After their removal it was found impossible to relieve the gall-bladder, held firmly by adhesions. I therefore decided to stitch its mouth to the parietal peritoneum, and to pack the gall-bladder with gauze, and allow it to remain until union would occur along the peritoneal sutures. This proved entirely satisfactory. The gauze packing was allowed to remain seventy-two hours. It was then removed, and a drainage-tube inserted. There were no complications during the progress of the case. Recovery satisfactory.

Differential Diagnosis.

Enlargements of the gall-bladder are chiefly dependent upon the existence of gallstones. The symptoms produced by them will depend upon whether they are free in the gall-bladder or have become impacted in one of the ducts. If no impaction exists they

produce little or no pain. When gallstones become impacted the symptoms will vary according to the part of the biliary duct obstructed. If the impaction be in the common or cystic duct a distended gall-bladder will probably be detected. If the impaction be in the hepatic duct there will be no tumor, and the patient will become jaundiced. If the obstruction exist in the cystic duct there will be pain and vomiting accompanied by subnormal temperature with an entire absence of jaundice.

The pain, severe at the onset, starts about the epigastrium, and radiates toward the right hypochondriac region, and upward toward the back of the chest and neck, but never downward.

Enlargement of the gall-bladder may be caused solely by the presence of gallstones.

Enlargement of the gall-bladder from accumulation of bile will depend upon obstruction of the common duct. If this accumulation is removed the tumor will disappear.

Enlargement of the gall-bladder from the accumulation of a serous looking fluid will occur if occlusion of the cystic duct exists.

Enlargement of the gall-bladder dependent upon suppuration as a result of the presence of biliary calculi, adhesions of the abdominal, and fistulous openings, may arise, and gallstones be extruded.

Enlargement of the gall-bladder from cancer may occur, and it is now admitted that gallstones are frequently found in association with cancer of this organ.

Operative Treatment.

This question may be considered, first, for the simple removal of stones from the gall-bladder without impaction ; second, removal of stones where impaction in the ducts exists.

To the first question the answer would be whether the risk of operative interference is less than that of allowing the stones to remain.

The necessity of surgical procedure becomes apparent, for the clinician has failed to present us a remedy which will increase the driving force behind the stone in the gall-bladder, or the pressure of bile. Neither has the clinician discovered a means by which the peristaltic movement of the gall-bladder or bile duct walls is increased, nor has he presented us with a method of dilating the bile ducts, and so lessening resistance.

Carlsbad treatment, vague massage, exercise, and strychnine have

all been tried, and are found to be non-curative. Medicine seems to be the only palliative.

We may feel safe in adopting the proposition presented by Mayo Robson, that as soon as gallstones give serious trouble their removal by operation is the rational method of treatment, for we may expect to find cholecystitis in every case.

Movable Kidney.	Tumor of Gall-bladder.
Most often found in thin women before forty.	Most often found in fat women after forty.
Health not seriously impaired.	Loss of flesh and strength.
Nervous subjects.	
Palpation.	
Swelling not easily felt with one hand on the anterior abdominal wall, and patient breathing quietly.	Tumor felt close to the anterior abdominal wall, and moves with respiration.
Pressure on swelling produces sensation characteristically disagreeable.	Tender when pressed upon.
Slips back into kidney region easily and completely, and remains there when patient is recumbent, unless disturbed by bodily or respiratory movements.	Has to be coaxed into kidney region, never disappears, and will not remain there unless artificially retained.
Freely movable in every direction.	Moored at its upper part.
Bimanual palpation (one hand in front, the other in loin).	
Movable kidney.	Distended gall-bladder.
Swelling flattened, solid, but not hard.	Rounded; feels very hard (because it is tensely distended with fluid).
Swelling jerked down by forced inspiration; easily detained by hands pressed together above or below it; apt to jerk suddenly back again when pressure is relaxed.	Tumor descends steadily and aggressively on forced inspiration; ascends vigorously on expiration against any obstacle.
Loin feels hollow when the swelling is in the abdomen.	Kidney may be felt behind, and independent of tumor.
Percussion.	
Tympanitic over swelling.	Dull over tumor; dulness continues with that of liver.
Malignant disease of the head of the pancreas.	
History.	
In gallstone case: pain first, jaundice second.	In malignant pancreas: jaundice first, pain second.
In gallstone: jaundice intermittent.	In pancreatic disease: jaundice steadily progressive, and deeper than in gallstone case.
Patients with gallstone may have a long history and be in fair condition.	With pancreatic disease he emaciates rapidly and dies soon.
Last, but not least, in a gallstone case the gall-bladder may be contracted and cannot be felt.	In pancreatic disease the gall-bladder can be felt as a distinct tumor, often of large size.

—Rutherford Morison.

From the reported cases you may be left to infer the method which I adopted. I will, therefore, not waste your time by referring to the complete technique, but will only speak of the position of the patient lying in the dorsal position, a firm, narrow sand-bag placed under the back at the level of the hepatic region, thus bringing the gall-bladder to the front.

The incision may be vertical or oblique ; the latter is preferred by Kocher. The operation should be completed in one sitting. Failing, we may be compelled to adopt cholecystectomy as introduced by Langenbeck, and afterward practised by Meredith, of London.

Regarding the gall-bladder as a diverticulum, being absent in a number of animals, it can hardly be regarded as a necessary viscus. I apprehend that when it is emptied of its contents during an operation, and before it is stitched to the parietal peritoneum, that it might be packed with sterilized gauze before stitching it and by allowing the gauze to remain twenty-four or forty-eight hours, thus affording time for union to the parietal surfaces, then removing the gauze and inserting a drainage-tube, thereafter treating the case as usually directed.

If a calculus be impacted in the ducts, cystic or common, and if it cannot be removed through the gall-bladder or dislodged with a scoop and forceps, aided by manipulation on the outside of the duct, an attempt to crush it would be justifiable. If all these fail and the impacted stone is fairly within reach the duct may be incised by a clean cut, the stone removed, and the cut closed by Lembert sutures, as was first practised by Henry O. Marcy, of Boston, on October 26, 1889. He says: "I then divided the walls of the common duct with scissors and everted its edges from over the roughened calculus, which was even then removed with difficulty." The thickened mucous membrane of the duct and bladder was joined by a fine, continuous, tendon suture, the patient making a rapid recovery.

While we admit that Lister made a reality of the hope which had sustained the surgeon's endeavors to remove the impenetrable cloud which had stood for centuries between great principles and successful practice, it would be well to remember that Dr. Bobbs's (of Indianapolis) operation preceded this great event. He it was that opened up another and wonderful field. Since then injuries of the liver are treated by operation, gall-bladders incised, calculi evacuated, and the gall-bladder removed when desirable.

DISCUSSION ON THE PAPERS OF DRS. DAVIS AND MYERS.

DR. JAMES F. W. ROSS, of Toronto, Canada.—Mr. President: I have been selected to open this discussion for some reason, I do not know exactly why, but my experience in gall-bladder surgery comprises about thirty-seven cases, which I expect will be published shortly. I hope some of the other Fellows will follow my example and publish their statistics on this subject, because it is something we want to get together.

I am pleased to know that it was in this town that Dr. Bobbs did his first cholecystotomy.

There is a great deal to discuss, and it is impossible to cover the whole subject. However, I would like to say that, from actual clinical experience, the experimental work carried out by Dr. Davis has been corroborated. I have had actual clinical experience that has satisfied me in this regard, that with Morison's pouch behind, a long glass drainage-tube, with iodoform gauze, we do not need to hesitate, in any case of gallstones in which we have severe adhesions, to operate. We should be able in some way to give these patients relief. I have one curious experience to demonstrate this fact: I operated on a lady in our city who had a gallstone in the common bile duct. She had been jaundiced for nearly a year; she was in good condition, but on opening the abdomen I found a sacculated condition of the gall-bladder that we sometimes meet with. There were several saccules with constrictions between, preventing the removal of the stone from the saccules beyond. Multiple incisions were required in this case. After several stones were removed from the saccules there was still a stone in the common bile duct to be dealt with, and I found it absolutely impossible to get at the common bile duct on account of the adhesions and the condition of the patient. I made up my mind that I must relieve her of jaundice in some way or other. Having had experience with two previous cases, in which I did an anastomosis of the gall-bladder with the colon, and in which the stone, though left *in situ*, had given rise to no further trouble, I came to the conclusion that if the bile ceased to flow over the stone that stone would remain, but would disintegrate. I was unable to do an anastomosis with the intestine, owing to adhesions; I could not get the gall-bladder up to the intestine to do an anastomosis. I therefore put into the pouch of Morison (a pouch which is as distinct as that of the cul-de-sac of Douglas) a

drainage-tube, packed gauze around it, had the nurse drain the tube frequently, so as to prevent extravasation of bile into the peritoneal cavity. If this drainage is carried out frequently you can insure the patient from peritonitis. The sinus drains occasionally when mucus plugs the common duct, but the jaundice has long since been relieved.

Dr. Davis has told us that the peritoneal cavity will tolerate a certain amount of bile, but not more than that. The doctor has proved by actual experiments what some of us have found out by clinical experience to be true. I might say in this connection that the patient I have referred to had bile discharged for some time. The tube was taken out, and an ordinary gum-elastic catheter passed in and left there. On some occasions a few drops of bile will come out, and then the bile passes down through the common duct, but the stone is either passed, as in the case mentioned by the essayist, or has become disintegrated as a consequence of the lessened amount of bile passing over it. In connection with that point I would like to say I have since had another case of anastomosis of the gall-bladder with the colon, and the patient is in perfect health. I have had three cases in our city with a large amount of bile passing into the colon, with the stone in the common bile duct still left behind. If a stone in the common bile duct can be removed, so much the better.

I am sorry that I did not bring with me a new instrument that I have devised for the purpose of raising the stone while in the common bile duct up from the hepatic artery and the portal vein. The portal vein and hepatic artery are more on a posterior level than the duct. To raise the duct with an instrument with sharp points is dangerous. My instrument with the little rollers rolls up the duct, holds it there, and the branches of the instrument are so arranged that they permit stitching of the incision that has been made in the duct between them. I have not had an opportunity of using it thus far, but it will take the place of the fingers, which in these operations become extremely tired.

The main point in the paper of Dr. Davis is the fact that, even though we are unable to close the wounds, we are still able to save the lives of patients by means of drainage-tubes and gauze packing, and that is very important.

Regarding the injuries of the biliary ducts, an important point which the doctor mentions is that we have no hemorrhage, but an extravasation of bile that produces peritonitis. It seems, therefore, advisable, in cases of punctured wounds of the abdomen, to do a celiotomy and ascertain the exact condition that is present in each.

Regarding Dr. Myers's excellent paper, he made a good point when he said that the pain is never downward, but is always upward; it is above the level of the umbilicus.

With reference to the diagnosis between gall-bladder trouble and movable kidney, I would like to point out that this differentiation was first given by the late Mr. Lawson Tait. (Here Dr. Ross demonstrated this diagnostic point by the aid of diagrams and sketches.) Gall bladder swings in the arc of a circle with the center above. Kidney swings in the arc of a circle with its center in the median line.

I must take exception to one point, that in obstruction of the common bile duct we have enlargement of the gall-bladder. That is not my experience. In chronic obstruction of the common bile duct we have a contracted, thickened and oftentimes adherent gall-bladder. In obstruction of the cystic duct we always have enlargement of the gall-bladder.

Regarding medicinal treatment, I quite agree with him that the amount of sweet oil and other nauseous drugs given in these cases would form a veritable materia medica monument; they do no good, and gallstones generally are not the innocent little things that they are supposed to be. There are a great many deaths from gallstones that are perhaps unrecognized. There may have been a previous inflammation of the gall tract spreading to the liver, a pylephlebitis occurring, and the death of the patient from prolonged fever scarcely understood. When a gallstone is demonstrated to be present, it is better to have it out than to leave it where it is—that is, with the exception of those cases where it occurs in the common bile duct and cannot be removed. Given a case in which we have an adherent inflamed gall-bladder below the surface of the liver, I claim it is difficult to diagnose it from an inflamed kidney. I have had one such case. Even after the abdomen was opened I was not certain whether I had to deal with an adherent, inflamed, thickened gall-bladder, or a displaced kidney. One minute I thought it was a displaced kidney, the next minute I thought it was the gall-bladder, and a third time I concluded it was the gall-bladder, opened it accordingly, and removed several gallstones.

It is really marvelous to look over the literature of the subject in the past ten to thirteen years to see what progress we have made. There has been no greater advance in any branch of surgery than in the surgery of the liver and of the gall ducts.

Dr. Robert T. Morris, of New York.—It seems to me we have been too much frightened by the elaborate methods and dissertations

with a view to preventing the entrance of bile into the peritoneal cavity. A small amount of bile in the peritoneal cavity certainly does not do very much harm, unless it is accompanied by inflammatory products from the lumen of the gall-bladder. If we have mixed infection, or colon bacillus infection, which is so common in cases of gallstones with empyema of the gall-bladder, then the infection follows the course of the leaking bile more rapidly, the bile acting as a vehicle for carrying infection. However, even in such cases, with a small drainage-tube which we can apply, surrounded by gutta-percha, I have lost my fear of drainage of bile into the peritoneal cavity from the common or the cystic duct, or from the gall-bladder, which cannot be sutured or drained properly. It seems to me that if we can comprehend fully the mechanical features involved in a small capillary drain; if we can keep a mass of fresh gauze on the outside of the abdomen, so that capillary drainage is going on continuously, a very small capillary drain will remove the bile as rapidly as it will escape from the cut cystic duct or common duct, or surface of the ruptured liver. This is not theoretical, but is based on my experience. In addition to the capillary drainage we must remember that we have atmospheric pressure, which in itself is sufficient to make the fluid take the line of least resistance, which is toward the surface along the line of the capillary drain. We need not fear, therefore, the danger of bile from the common bile duct or cystic duct that we have been led to believe from elaborate and voluminously written articles upon the subject by surgeons who have been quite properly very careful for the sake of their patients. I have written a good deal about the small drainage-wick which is used for this purpose, with which most of you are familiar. It is flexible and adapts itself more readily to the movements of the liver during inspiration and expiration than a glass drain-tube. I have used the glass drain-tube and rubber-tube, and I find patients complain a good deal because on inspiration the free edge of the liver is carried against it forcibly, it being a cause for discomfort or distress. A soft, flexible drainage-wick will enable us to drain off the bile as fast as it escapes.

Another resource which I have employed has been suturing of the lesser omentum to the distal extremity of the abdominal wound. That makes a little trough for the bile in the cases in which it can be done, but in many cases the lesser omentum can be sutured to the lower distal margin of the abdominal wound, then our bile and other fluids will drain to the surface almost without a drainage apparatus.

In a case in which I wish to replace the gall-bladder I have adopted a resource which I think will be of service in a certain pro-

portion of cases in which we separate the gall-bladder from the adhesions, and in which the muscularis of the gall-bladder is not too much thickened by infiltration—that is, the inversion of the fundus of the gall-bladder after suturing in such a way as to obliterate this cavity. (Here Dr. Morris demonstrated this point by diagrams and blackboard sketches.)

Referring to the paper of Dr. Davis, I have had only two cases of rupture of the liver. One patient was kicked by a horse at close range, with great force, so that about one-third of the liver was destroyed. I determined by examination at the time of operation, and by percussion since that time, or since the patient's recovery, that the liver was rent in many directions. If I am not mistaken, several pints of serum, bile, and blood were found walled in by new adhesions in one mass. This was evacuated, and a number of fragments of the liver as large as the small bottles upon the table here. I tried to separate the adhesions and found hemorrhage was free. They were necrotic, so I digested them with pepsin. The new adhesions protected this region. The pepsin was poured in two or three times a day, and it took about a day and a half to digest all of them. They were all liquefied with pepsin, came away, and left the surface of the liver quite clean, and granulations began to take place. There was very little infection. The patient recovered and is now living without a great amount of liver left.

The other case is one in which I operated for a perihepatic abscess resulting from acute appendicitis, with very extensive adhesions. In separating or splitting the adhesions to get at the appendix, which was beneath the liver, I had split off quite a slice of liver; it bled quite freely. I paid no attention to direct treatment of the bleeding surface, simply used a small capillary drain, and allowed the hemorrhage to ooze. I believe we had a small amount of bile with the discharge for two or three weeks. The fistula remained open for a long time, and as the case is a recent one it has not closed yet, but no harm resulted from the free oozing from the ruptured liver surface.

Dr. Joseph Eastman, of Indianapolis, Ind. (by invitation).—In the *Transactions of the Indiana State Medical Society* some years ago you will find a report by Dr. Kemper, of Muncie, where an enormously distended gall-bladder pushed up against the abdominal wall, became adherent, and the pus and gallstones contained within the substance of the gall-bladder sloughed their way through and were discharged externally. If Dr. John S. Bobbs, of this city, had been my father, I would on this occasion say no less in the interest of truth, not wishing to injure his good name nor his reputation, as he was a

great surgeon and a great character; but so far as I can learn from persons who were present at the operation for which he has the credit, June 15, 1867, there was nothing between the case of Dr. Kemper and Dr. Bobbs except incision with the knife; the gall-bladder was up against the abdominal wall and adherent; it was already bulging and fluctuating and almost ready to burst. He operated for what he and his patient believed to be an ovarian granule; gallstones and serum poured out. The wound was large, the gall-bladder was adherent; his sutures were only to close the wound in the skin; he used few sutures. Today this operation is by praise distorted. I only speak of this, not to cast any reflection upon Dr. Bobbs, but to state the facts as I obtained them from gentlemen who were present at the time.

I have been very much interested in the subject of gallstones, and must compliment Dr. Myers upon his very scholarly paper, as well as Dr. Davis on his very interesting and practical experiments on injuries of the liver. They all come together in connection with the subject of surgery of the liver.

With regard to the question of diagnosis, I have had two cases which put my methods of diagnosis somewhat to a test. It is well known that in obstruction to the gall ducts the liver becomes enlarged. I have never yet seen a case, where gallstones had obstructed the ducts, in which the liver was not enlarged to one-third or three times its normal size. In a case that came under my care the left lobe of the liver was enlarged, the right not at all. As a result of that, the gall-bladder was swung around to one side, and I cut in through the back. I think my friend Dr. Sutliff thought we were cutting for a pyonephrosis. After cutting in through the back or well backward, out came gallstones and pus, and we found the gall-bladder had been switched around and really occupying the space below the right kidney.

Another case, Mrs. G., I was asked to see a few months ago. The case was considered one of appendicial abscess. There was an enormous enlargement found in the region of the appendix, which fluctuated. It was close down to Poupart's ligament. Her condition was too critical to do anything at the time, and in a few hours we found there was a shrinkage in this mass and a better condition of the circulation, enough to encourage us to wait. We waited for nearly a month, at which time it was above the umbilicus, and upon opening the abdomen we found the gall-bladder largely distended with pus and gall, with a large gallstone impacted in the duct. Of thirty-five cholecystotomies which I have done, and one cholecystectomy, I have never yet found a gallstone in the common duct; if I had I would

confess it, as I have no object in making a statement to the contrary. I am led to believe, therefore, that many of the gentlemen who say they have found a stone in the common duct are mistaken. I cannot believe a patient will live very long with a stone in the common duct, absolutely deprived of the secretory and excretory functions of the liver. I would like to hear from some of the other gentlemen on this point.

In going through the wound in the top of the gall-bladder I have time and again found such a pressure of the gallstone in the cystic duct as to obstruct the common duct by a sharp flexion, causing a localized peritonitis, with adhesions and thickening of the parts of the mesentery and omentum; but I have never yet found a gallstone in the common duct. Why it is that my experience differs so from other gentlemen, I cannot say. If I have ever left a stone, I do not know it. I invariably pass my fingers well up under the liver, feeling carefully all over the ducts, so that I can detect a gallstone if it is in the common duct.

With regard to inversion of the gall-bladder, I would not advise that procedure from my experience. I was trying to recall the name of a German surgeon who reported some time ago a number of post-mortem examinations after cholecystotomy, and he states that the gall-bladder becomes a mere stringy, functionless bit of tissue, and therefore there would not be any possibility of its being filled up again with gallstones.

I am much in favor of cholecystotomy, for the reason that the liver is always enormously enlarged; it shrinks rapidly, and the general health of the patient becomes much improved, and for the reason also that with obstructed gall ducts and a hyperemic liver the stone may have a causative relation to cancer of the liver; the removal of the congestion, then, certainly has an influence in preventing cancer of the liver.

Dr. Frank C. Ferguson, of Indianapolis, Ind.—I have here a gall-bladder full of biliary calculi, which I got from a patient post mortem. I was called to see the case in consultation, but the patient died before my arrival. The gallstones are still *in situ*, but you can see their immense size. It must have required months or years for these calculi to have grown so large. The woman suffered, for many years, from periodical attacks of paroxysmal pain, vomiting, etc. During the last attack the gall cyst ruptured, bile was precipitated into the peritoneal cavity, and she died of septic peritonitis. The specimen is a valuable object lesson to every physician. This patient's life could have been saved by a surgical operation at any time prior to

the rupture of the cyst. It teaches us that delays are dangerous in cholelithiasis as well as in appendicitis.

Cholelithiasis is a surgical disease, and should be treated by the surgeon.

I have been greatly interested in the discussion of this subject by this society, and am somewhat surprised that so many advocate cholecystotomy as the ideal operation. I have had some experience in operating for gallstones, and am firmly of the opinion that cholecystenenterostomy when done with the Murphy button is the ideal operation. It is quickly and easily performed, is attended with little danger to life, the case is permanently cured, convalescence is rapid, and the patient is not annoyed by a filthy and loathsome biliary discharge. Among several cases upon which I have operated by this method, two are worthy of special notice, and I show you here a Murphy button that did duty in these two cases.

The first case was a woman, aged thirty-two, who was four months pregnant at the time of operation. She was deeply jaundiced, and suffered almost continually from hepatic colic. I removed thirty-two gallstones from the gall cyst, and anastomosed it with the colon, using this button. The button came away on the sixteenth day, and the patient returned home soon afterward. She gave birth to a living child at full term. She has had no symptoms of gallstones since the operation, more than one year ago, and her digestive and bowel functions are perfect.

The other case in which the button was used was of twenty-years' standing. She had been confined to her bed for eight weeks previous to the operation. I removed 302 gallstones and anastomosed the gall bladder to the colon, as in the case just mentioned. The button came away the tenth week. She now has good health, and has had no symptoms of the disease since the operation. The gall cyst in this case was enlarged, and its walls were thickened. This, doubtless, accounts for the long sojourn of the button in the intestinal canal.

I am well aware that some cases occur in which the gall-bladder is so diseased that its anastomosis with the intestine is impracticable. Such cases have occurred in my practice, but judging from my own experience and the experience of others, cholecystenterostomy, when it can be done, is the ideal operation.

Dr. Willis G. Macdonald, of Albany, N. Y.—There may be a few things to be said, even yet, notwithstanding the careful discussion the subject has received. There can be no question but what gallstones do lodge in the common duct. I have myself operated upon a case in which I removed gallstones through an incision in the duo-

denum and dilatation, with some little deviation of the opening of the common duct into the duodenum. Dr. McBurney has also reported two or three such cases.

In relation to the operation of incision and suturing of the common duct, which has been referred to as having been invented by Dr. Marcy, of Boston, there is some question of priority. His case must have been reported first, but in 1887 I saw Dr. Vander Veer open the common duct, and, while he did not suture it, he placed a drainage-tube directly into the opening, packed about it with gauze, and the patient recovered. A little later I saw a surgeon do the same operation in the city of Berlin, so there is some question with regard to priority in this matter.

With reference to the medical treatment of gallstones, I do not deny that sweet oil may be of some use. In places it is regarded as of some therapeutic value, as well as other remedies in similar conditions. After the administration of olive oil in several cases of gallstones, I have seen them come away just the same as we take them out with a knife. There is no question about it. I remember one particular case where two hundred calculi were expelled, and in another case I was deprived of an excellent fee by the beneficent influence of olive oil. I think I have seen good come from the administration of acetate of iron in relatively large doses, say twenty grains, four times a day. I do not believe that the larger percentage of cases can be cured by the administration of remedies such as I have mentioned, but I know the symptoms of gallstones can be relieved by the internal administration of olive oil. It is a plan worthy of employment.

I want to join in agreeing with nearly all of the gentlemen who have spoken this afternoon, that if sufficient drainage is established in non-infectious cases there is very little harm which can come from the pouring of a little bile into the peritoneal cavity. I have felt in later years that an oblique incision following the border of the ribs furnishes certain facilities in the way of drainage over the one which is perpendicular, following the external border of the muscle, and that we do have here a cavity, which has been described by Morison, which, with very little assistance, will take care of a good deal of bile, and also contains a certain number of pathogenic bacteria.

With regard to lacerations and wounds of the liver, I have had two experiences—one unfortunate, both very similar—and were operated on within twelve hours from the time of the receipt of the injury. Operation was done largely as a matter of exploration. One man had been caught between a boat and the landing and in some way caught his body and caused the injury. The other patient, a boy, had been

run over by a loaded beer truck. In the boy's case these was a laceration which almost divided the right lobe of the liver obliquely across it, and there was an immense outpouring of blood. In that case I put in a few sutures in order to arrange a sort of border to do my packing behind it. In suturing I used No. 5 catgut and a perfectly round darning needle. It was of some assistance. We got nearly all the benefit from it by the pressure of iodoform gauze. We are in the habit of making our own iodoform gauze in the clinic, for the reason that we do not have very great faith in the gauze of manufacturers. It is prepared in pieces and put in a formaldehyde sterilizer for the purpose of sterilization. The little formaldehyde which seems to lodge and remain in the iodoform gauze helps us out wonderfully, besides the pressure in helping to control the flow of blood from a large oozing surface. It seems to glaze over. In wounds of the liver it is undoubtedly of some value, yet in packing against the intestine it is very disadvantageous.

I never have tried very hard after my first efforts to suture the bile ducts, and, from the experience of other men, I am not going to try very hard to do so in the future. While we have the Halsted hammers and a variety of other things which have been invented to assist in doing this kind of thing, yet, after all, they are largely ornamental and lie on the shelf in the operating case.

With regard to leaving gallstones lodged in the common duct, such a procedure has been advocated by Dr. Ross in a certain class of patients, particularly those who have been deeply jaundiced for a considerable length of time. Where it seems very desirable to have the briefest possible anesthesia, not expose the patient any more to the dangers of shock and sepsis than is possible, I believe this is one of the most important and best life-saving processes with which I am familiar. The only difference is, that I prefer, if I can, to get hold of the jejunum and attach it to the gall-bladder instead of the colon.

Dr. J. Henry Carstens, of Detroit, Mich.—Last year I had a case sent to me which I supposed was a diseased kidney, probably malignant. There was a fairly large, bulging tumor. I cut down, but could not make out anything. Everything was inflamed and adherent. I kept on dissecting and dissecting for about an inch and a half, when suddenly a little bile made its appearance. I followed the opening and found some gallstones. I did not suspect gallstones. The walls of the gall-bladder were an inch and a half thick; I never saw anything like it. The gall-bladder was bigger than my fist, and the exudate and inflammatory products around it were immense. Dr. H. O. Walker, who was present at the operation, had a good laugh at my

expense. I told him that I was not infallible—that I was liable to make mistakes. Within two weeks thereafter my friend Walker had a clear case of gallstones and asked me to come and see the operation. I went, and at the operation he did not find any gallstones, but simply an enlarged kidney. It became adherent, pressed on the gall duct, and produced jaundice.

I have simply mentioned these two cases to show how difficult the diagnosis is, and that we do not want to be too positive. I cannot agree with Dr. Ross about leaving these gallstones in. May be there are cases where it is impossible to remove them. When we have to loosen so many adhesions, if a patient can stand the anesthetic long enough for us to stitch the gall-bladder to the intestine or transverse colon, she can also stand the anesthetic a little longer, so that we can dig down and remove the gallstones, which takes but a few minutes, and put in a gauze drain. You are then sure you have removed everything.

When I am called to the country to see a case the attending physician gives me the history, and I make a few inquiries and then tell the doctor that the patient has gallstones; he is very apt to say: "Good heavens, you don't say so; it is not possible; the patient has not had any jaundice." And I tell him that jaundice is not always indicative of gallstones. Here is the trouble with the teachers on the practice of medicine, and also our text-books on the practice of medicine. They seem to emphasize very strongly the point that there must be jaundice. The fact of the matter is, that a great many people suffer from gallstones who never had any jaundice, never will have, and still have gallstones.

I recall a case similar to the one narrated by my friend Dr. Macdonald, where a woman had passed gallstones continually. She told me she was suffering from gallstones and showed me a bottle full of small ones. She said she had taken sweet oil, but she had a lot of them in her yet. She expressed her desire to have an operation. She said she knew one had become stuck fast and could not get out. Now, that woman had good horse-sense, and I removed the remainder of them by surgical interference. Simply because a woman passes a lot of gallstones there is no reason why she will pass all of them by taking sweet oil or any other remedy. Before taking my seat I desire to emphasize the point that our text-books on the practice of medicine should state emphatically that it is not necessary to have jaundice in order to have gallstones.

DR. M. ROSENWASSER, of Cleveland, Ohio.—In connection with the treatment of gallstones by the use of olive oil, I would report the result

of a case so treated by a physician in Frankfort, Germany. This practitioner discarded the use of olive oil by the mouth, but administered it by means of injections into the rectum. He had a table made especially for this purpose. The patient was placed back uppermost in the Trendelenburg position, a quart or more of olive oil was injected into the rectum, and the patient left in that position for about an hour. He seems to have been so well satisfied with the success of this treatment that he published a report of six cases treated by this method after other methods had failed. The case I have in mind was a lady who was reported among the six successfully treated. She had undergone this treatment and had been relieved for six months. She finally came to this country, and was taken with jaundice and symptoms of localized peritonitis. I operated on her for empyema of the gall-bladder, removed seven stones from the gall-bladder and cystic duct, and now she is perfectly well. I mention this case to show that we cannot be too careful in deciding whether a given remedy effects a cure because the patient improved after its use. The patient may have recovered despite its use.

Dr Davis (closing the discussion on his part).—I have enjoyed the paper of Dr. Myers very much. Dr. Eastman spoke of the character of the operation done by Dr. Bobbs, who performed the first operation for gallstones. It has been my impression for years that he operated through a mistaken diagnosis, and that the gall-bladder had become attached to the abdominal incision. Marion Sims did the first planned operation for stone in the gall-bladder, and it was the classical cholecystotomy. Very little advance has been made in the operation since Marion Sims performed it in 1878. Tait operated very much as he did. We may say that the surgery of the gall-bladder is about settled. Very few points remain in question in that department of the surgery of the biliary tract. Those who close the incision in the gall-bladder, dropping it back into the abdomen, will find in some cases they have left a stone behind. It is very easy to overlook a small stone, which may cause trouble afterward. It is also better to attach the gall-bladder to the abdominal incision, as the drainage assists in effecting a cure.

Dr. Morris spoke of using a capillary drain in those cases where he operates on the common duct with extensive adhesions. If you go behind the liver and find no adhesions, it is evidence generally that you have no stone impacted in the duct. When you break up adhesions these cases bleed freely. The gauze is very helpful in controlling the hemorrhage; it walls off the field, and I cannot believe that a capillary drain has the advantages of the gauze packing. Up to 1892

no cases had been operated on that I could find where the duct was not stitched. I read a paper before the American Medical Association in that year, in which I took the position that it was not advisable to attempt to stitch the duct; that these patients were usually very cholemic, and lengthy operations were not stood well. I held at that time that it was best to make an incision in the duct, remove the stone or stones, introduce a glass tube, and pack around the gauze. Since then a large number of operations have been done, and the reports have been satisfactory.

Dr. Morris spoke of invaginating the gall-bladder. When we remember its anatomy we must understand that there are very few cases where he could do an operation like that. The gall-bladder would have to be very much enlarged, hence this operation would be impracticable, as in the great majority of cases, where there is obstruction of the duct, the gall-bladder is very much reduced in size. I recall one case on which I operated where the gall-bladder was not as large as the end of my thumb and buried in the liver tissue. The liver will envelop the gall-bladder in many places, and you will have to enucleate it.

I was very much interested in Dr. Ross's ingenious device for elevating the duct, and I have no doubt it will be of great assistance in the operations for which he recommends it.

Dr. Eastman spoke of not finding a stone in the common duct, although he has operated thirty-three times. This is easily understood when it is remembered that in only a small percentage do we find a stone in the common duct. He may find one in his next case.

Dr. Eastman.—How long do you suppose a patient would live with a stone in the common duct?

Dr. Davis.—In the majority of cases these stones are not impacted and are movable. Patients may live for many weeks with stones impacted in the common duct.

I spoke of incising the liver where I was unable to find the obstruction. In some cases of very much enlarged liver, of which Dr. Eastman has spoken, the liver comes down a great distance, and we are unable to find the obstruction in the duct and the gall-bladder, which is contracted. I believe this procedure should be adopted in such cases—namely, a free incision in the liver, severing small biliary canals in the liver tissue, followed by packing with gauze. In this way I have no doubt cholemia will be relieved in some of the cases. In only one case did I operate with that end in view.

Dr. Myers (closing the discussion).—I shall occupy but a few moments of the time of the Association in closing remarks. Referring

to the history of these operations, it is not unusual to hear it said that Columbus did not discover America; that Shakespeare was written by Bacon, as is claimed by a man in the North (Ignatius Donnelly); that anesthesia was not discovered by Morton and Warren, but by a man down in Georgia (Long). This is merely a repetition of the same thing.

The remarks that have been made about sweet or olive oil are about as reliable as the reports we get from Christian Science, and you know we get any kind of reports from them we wish. Personally, I usually perform cholecystotomy, believing it to be a better operation.

Speaking of the operation of Dr. Bobbs, or of his discovery as being accidental, how long did it take us before we discovered that the peritoneum could be cut with impunity, if we had a clean knife and a clean touch? Accidentally some man made this kind of cut, and since then we have no more fear of wounding the peritoneum than we have of any other tissue in the body. Before that we stood aloof and saw in our mind that maxim "All hope abandon ye who enter here!" That was the position I was in when I graduated. I am very much obliged for the interest shown in my paper.

HEMORRHAGE AND THE MENOPAUSE.

By JOHN MILTON DUFF, M.D.,
PITTSBURG.

My observation teaches me to believe that there is a very general belief among the laity that hemorrhage is a necessary concomitant of the menopause. As a consequence of this belief many women welcome the foul discharges and hemorrhages sometimes accompanying this eventful period. They are considered as messengers bringing the joyful tidings of relief from menstrual vicissitudes and of the passing away of the child-bearing period. It has for years been to me a great source of pain to contemplate the misery and hopelessness of many of the wrecks of womanhood who have thus been deluded, who have finally presented themselves to me for relief when malignant disease had advanced to that stage where operative procedures did not present a single ray of hope.

Since the last meeting of this Association eleven women, all of whom were thus deluded, have come under my care for treatment when disease had advanced to that stage where no possible relief could be offered. During this same time twenty-seven others came under my care, most of whom were likewise encouraging themselves, but fortunately sought counsel before it was too late. Eight of these twenty-seven were suffering from well-defined malignant disease, sixteen from uterine fibroids, and in three I was unable to make a diagnosis. One of the malignant cases was only thirty-one years of age, yet she thought the change of life was working on her. Several of the others had not arrived at the proper age for the menopause, but nevertheless thought it was probably the trouble. I quote these because of their corroboration of the popular belief regarding hemorrhage and the menopause.

I appear before you to-day to discuss this subject, not because I have anything new to offer, but with the hope that I may awaken a new interest in it and help to instruct the public by aiding in a greater dissemination of facts concerning it.

I do not believe hemorrhage is a necessary concomitant of the menopause. When it does occur otherwise than as a menorrhagia in the true sense of that term, it is in 95 per cent. of the cases significant of a pathologic condition. A physician, to my mind, is not performing his duty to himself, to his profession, nor to his patient, when called to a case of hemorrhage of persistency, if he contents himself with a diagnosis of "change of life" and prescribes accordingly, without making a most careful physical examination of the genital organs. He should ask for the privilege of an examination, and, if denied it, he should demand it. If then refused, no sympathy, no pecuniary considerations should keep him from withdrawing from the case at once.

It is unnecessary for me to take up the time of this Association with statistics in support of my position. During the past three years I have interrogated, or had others to do it for me, 482 healthy women over fifty-two years of age. Of these only 39 gave a history of what could be termed a hemorrhage during the menopause, and out of this number only 5 gave a history of hemorrhage of any persistency. In 187 tabulated cases between forty-three and fifty years of age suffering from uterine hemorrhage, 19 were due to pregnancy, 48 to malignant disease, 53 to fibroids or uterine cysts, 10 to diseased endometrium, 26 to diseased appendages, while in 31 no positive diagnosis was made. In view of such statistics I cannot see how any physician can argue, as I have heard some do, that hemorrhage during the menopause is not, as a rule, significant of disease.

On the other hand, we must not be led by the absence of hemorrhage to feel secure in negative diagnosis of pathologic conditions. Some of the most malignant cases I have seen never gave a semblance of hemorrhage. I have also seen fibroids, large and small, and no hemorrhage present.

I may be thought too radical in some of my views. I have all possible respect for the feelings of woman. I honor her virtue and modesty, and would never intentionally subject her unnecessarily to an examination. I believe, however, that at any time of life where abnormal conditions arise in a woman an immediate examination should be made. Hemorrhage or no hemorrhage, apparently healthy or otherwise, it could positively do no harm for every woman to subject herself to an examination at intervals during the supposed change of life. I presume there is not one in this presence to-day who has not been called in to see women who

have suddenly become alarmed at their condition. They had not noticed anything abnormal, for, in many instances, more than a few years or weeks, and yet you have discovered that disease has made progress to an extent which would warrant you in the assertion that it had been of long standing. I am not one of those who believe that malignant disease of the genitalia can always be cured if seen early. I am positive, however, that in a very large percentage of cases an early operation will at least mitigate suffering and greatly prolong life. I think my experience will almost justify me in saying that they are sometimes cured, as I have one patient living eight years after a vaginal hysterectomy for cancer, two over six, two over five, three over four, and at least twenty over three years, unless some of them have died without my knowledge. At the present time it is very seldom that I operate upon an advanced case of malignant disease of the uterus and surrounding parts. I think, as a rule, it is useless and a discredit to surgery. I do not know of a single case that I have thus operated upon that has not had a return in less than a year. I do not know of one that has lived over eighteen months, and most of them have died in periods ranging from three to nine months after the operation. I know it is sometimes exceedingly difficult to make a positive diagnosis of malignancy early in the case. I presume some of my cases operated upon were not malignant. If they were not I scarcely regret it, for they were very suspicious, and they fully recovered from the operation. A few years ago I saw a woman with supposed malignant disease. Two of the best microscopists then in our city examined specimens from her. One declined to give an opinion, the other stated positively it was malignant. The woman concluded to await developments. She is living yet and well to all appearances. To me it is a question whether or not, even at the hands of the expert microscopists of the present day, it is always possible early in the disease to make a positive diagnosis. Happily, however, the clinical history and gross appearances are nearly always such as to enable us to make a diagnosis. In conclusion, I say emphatically: "All persistent hemorrhages during the menopause should be regarded with suspicion."

DISCUSSION.

Dr. D. Tod Gilliam, of Columbus, Ohio.—I am very glad, indeed, Dr. Duff has written upon this subject. His paper will do a great deal of good, provided it is distributed among the members of the general profession. Such a paper is not intended for this Association so much as for the general practitioner, because every Fellow here is fully aware of the importance of hemorrhage coming on at irregular intervals, or in such quantities as would be abnormal in woman. The general profession are not alive to the importance of the subject, and surely the laity themselves are very much at fault. It is a very common thing, as Dr. Duff has said, for a woman to regard a case of extraordinary bleeding from the genitals with perfect complacency. She regards it as the harbinger of a change which a great many look forward to eagerly; but in 95 per cent. of the cases these hemorrhages are indicative of pathologic changes. I do not believe it is possible to have an abnormal discharge of blood from the uterus without a pathologic cause, either systemic or local. Sometimes these changes are difficult to comprehend. Sometimes, with the most careful and scrutinizing examination, we fail to find what the cause of the hemorrhage is. But there is a cause back of it, and it cannot be physiologic when hemorrhage comes on in this way. We find many cases in which the hemorrhage is indicative of malignant disease or of fibroid degeneration. In other instances it is due to a pathologic condition of the endometrium itself; in still others it is due to obstruction of the circulation on the outside of the uterus, such as abnormal growths, abnormal position, by which the vascular supply is interfered with. We find other cases due to the condition of the kidneys; others due to systemic causes, such as leucocythemia, etc.; but these patients ought always to be looked after carefully in order to determine what the nature of their trouble is.

I believe with Dr. Duff that in advanced cases of cancer of the uterus there is very little to be done to prolong life. I have very little faith, in advanced cases of cancer of the uterus, of doing anything to eradicate it from the system entirely. Every case I can get in which I think I can remove the uterus safely, where I suspect cancer, I do so. The suspected cases do not always prove to have been malignant in character. I recall one particular case in which, before the removal of the uterus, I took away a handful of detritus which presented every indication of cancer. I expected the woman to die. She was pale, anemic, and run down when she came to the hospital. I expected to lose her within three or four months after the operation. I sent the uterus to a pathologist for examination, telling him to be careful to take his

sections from the sides of the uterus, to see whether the disease had extended to the broad ligaments. Instead of confining his sections to the side of the uterus, he took sections from the cervix adjacent to the part I had removed by the curette, and much to my surprise he reported "no cancer." I made this a text for my lecture before the students, to show that the average microscopist cannot tell whether cancer is present or not. While I did not want to ridicule microscopy, I wanted to impress upon their minds the fact that they could not always depend upon the microscopist for a positive diagnosis in these cases. One year, even three years, went by, and the woman still remained in perfect health. I believe I can now consistently give up the idea that that was a case of cancer. What the case was I do not know, but it resembled cancer very closely in its macroscopic appearance and clinical features.

I think Dr. Duff's paper will prove of vast benefit. We cannot disseminate the idea too widely and impress upon the general profession too strongly that a uterine hemorrhage, abnormal in quantity or abnormal as regards time, is always indicative of some pathologic condition, and that the patient ought to be sent as soon as possible to a physician who is capable of making an intelligent examination, with a view to determining her real condition.

Dr. T. J. Crofford, of Memphis, Tenn.—This is a subject of great interest to me, and I was much gratified to hear the essayist say that he regards these hemorrhages in a large percentage of the cases as being due to pathologic conditions. I agree fully with Dr. Gilliam, that they are all due to a pathologic condition. The Doctor said except a few cases of menorrhagia. I suppose he had reference to structural changes. It is not necessarily a structural change in the uterus or endometrium which causes it, but there is a pathologic condition of some sort in all cases. Menorrhagia is a symptom. When a woman does not menstruate properly, there is disease in some way or other.

While a paper like this does much good before a body like this, it would do more good if presented before a general society. About two years ago I was asked to prepare a paper for the Texas State Medical Society. I live a good ways from Texas, and could not attend the meeting. However, my paper was published in the *Transactions* of that Society in 1898. In this paper I covered practically the same ground as Dr. Duff did, except I said that the general practitioner is the one who is more responsible than the specialist or gynecologist, for the reason that he gets the case first. The general practitioner should be well informed on these points. While some general practitioners are excellent physicians, yet a large percentage of them dismiss these cases with very little thought, calling it the "change of life," and as a conquence the woman goes on having hemorrhages until she becomes

incurable. There are some general practitioners, as well as specialists, who are familiar with these diagnostic points and investigate their cases closely, and then, if necessary, refer them to a competent source for prompt attention.

With regard to these hemorrhages and cancer, I doubt very much if the statistics of cancer of the uterus can ever be made good, even with all the alertness and watchfulness that the profession are capable of giving it. When we consider the shape of the uterus, the small size of the neck, wedged, as it is, between the bladder and rectum, how quickly the disease can spread from that narrow neck, how rich in lymphatics the tissue is surrounding it, how readily it can be carried into the broad ligaments or around the uterus, we appreciate that it cannot be removed even by a hysterectomy. We appreciate, too, how the disease must be fatal if the organ be not removed very early.

We speak of recurrences. This is erroneous. It is a continuation of the disease, for the reason that it has not all been removed. I have been interested in watching the work of Jacobs for the last few years. He is thoroughly disgusted with operations for the cure of cancer. Jacobs is a fine manipulator, and performs vaginal hysterectomy better than anyone I have ever seen. At the time I saw him operate, two years ago, he told me that he had had only one or two cases that escaped for any long period of time without a continuation of the disease, or a recurrence, as some surgeons prefer to call it. Olshausen, Martin, some of the German surgeons, and some of the Austrians give better reports, so far as ultimate results are concerned, than does Jacobs. Those results are better than we get in this country. I have thought it possible that the reason for it was the form of government the country has. Belgium is practically a republic, and there is a sentiment against operative work pretty much like that which obtains in this country. In Germany, on the other hand, there has been a kind of coercion to make the people come to the clinics for a long time. The women realize that they get the best treatment at the hospitals, and they have been educated to go to the clinics, where eminent surgeons are in charge. The result is that in Germany and Austria, as soon as a woman finds there is something wrong with her, she goes to the clinic. The surgeons get their cases early, and I believe this is a reason why their ultimate results are better than in this country, Belgium, and some other countries.

In my opinion the sentiments the doctor has given forth in this paper will do a great deal of good if properly disseminated. I trust many general practitioners will read it. Women should be taught that the menopause means cessation rather than an increase of flow, and unless there is something wrong she will shed that function as gently as leaves will shed from a tree when the time comes. She

should be taught that. The general practitioner should realize that his responsibility at this time is greater than he generally supposes it to be. He sees these cases first, and should realize that there is no such thing as an increased flow at this time of life unless there is something wrong. If the Fellows of this Association will prepare papers of this kind and read them before their State societies, throwing the responsibility on general practitioners, letting them know that a great duty devolves upon them, it will do a great deal of good.

DR. WALTER B. DORSETT, of St. Louis, Mo.—I did not have the pleasure of hearing the first part of Dr. Duff's paper, but there were some statements made by him that impressed me very forcibly. I think that possibly we had better go one step further in regard to enlightening the general profession, and that is to take the subject up more in detail and make it more impressive upon students. The professors in our medical colleges ought to dwell more particularly on the subject of symptoms in regard to cancer at or about the menopause. It is not unusual to have cases brought to us from remote districts in which we have to tell them that they will have to go home without an operation. We are placed in a position in which we must simply read the death-warrant, as it were, as we cannot get around it. We cannot offer the excuse that has been offered by the family physician, but we must say positively to the woman that she must go home without an operation. Possibly Mr. Tait has been more or less to blame in the education of the profession in regard to the symptomatology of cancer. You will remember that in his first book he made a declaration like this (and I believe that I can almost quote his words): "That of all the diseases to which flesh is heir, there is nothing more painful than cancer of the uterus." That has not been my experience. I have seen a number of cases in which hemorrhage has been the principal symptom and of which the patient complains, and pain, except in very advanced cases, was almost always absent. This is particularly true in disease of the cervix rather than of the body of the uterus. I I believe that pain in disease of the body of the uterus is a very much more frequent symptom than it is in cancer of the cervix. This was very well impressed upon my mind when one of my confrères (Dr. Mooney), who about two years ago, just before he died, placed in my hands a patient whom he had curetted, and asked me to wash out the uterus now and then. She was a woman fifty-two years of age, and the principal symptom of which she complained was pain, and she plied me with many questions as to the cause of her having so much pain. I went to see the doctor in regard to the case, to obtain his permission to give her the necessary treatment, and to learn from him what he had suspected the case to be. I found him so ill that I did not feel like talking with him in regard to the case. The woman

went home, and a short time afterward I received a letter from her daughter, asking me to give a positive opinion regarding her mother. I asked her to correspond with the family physician, which was done, and I then wrote him what my suspicions were. This, of course, made the daughter very angry. She wrote me a letter, in which she said that I had done her mother a great injustice in not telling her the true nature of the disease, inasmuch as it had now become so far advanced that nothing could be done. She was sent to a physician in St. Louis, who extirpated the uterus, and he told me that when he began to do it (and this was only two months after the death of Dr. Mooney) the uterus came away in bits; he could hardly get his tenaculum forceps to hold a piece of it on account of it being so brittle.

I operated on a woman forty-eight years of age, in whom the appearance of the cervix was nothing unusual. Scrapings taken from the fundus of the uterus showed cancer by the microscope, and I extirpated the organ. I was surprised to find the amount of disease, according to the microscopic appearance, on the inside of the fundus of the uterus. If the scraping had not been done the body of the uterus would have been perforated, so extensive was the diseased condition of the fundus of the uterus; yet, at the same time, there was very little discharge, and the amount of blood that was lost was very slight. Pain at the fundus of the uterus is to be considered with a good deal of suspicion and as indicative of a pathologic condition. I do not believe we have pain so much in cases of cancer of the cervix as we do of cancer of the fundus.

Dr. Willis G. Macdonald, of Albany, N. Y.—I would like to inquire how large a proportion of the bulk of the profession of the different cities and country towns really attend their local meetings? I appreciate as well as anyone the value of Dr. Duff's paper. The general practitioners are the men we want to reach, but in a very great many localities only a small proportion of the local profession attend their county societies, academy of medicine meetings, etc. The trouble is that there are a class of men in the medical profession who do not attend the meetings of their local societies, either county, State, or district. Those are the men who are yet telling their patients that an abnormal flow of blood from the uterus is due to a change of life, and will disappear after a year or two; those are the men who treat acute and fulminating appendicitis with opium. It seems to me that Dr. Dorsett came nearer the solution of the thing when he said that we should teach our young men who are to go out from our colleges and occupy places which are now occupied by the older men, who have so far drifted away from medical interests, because they have so many social and business interests that the practice of medicine is an incident rather than the chief object.

DR. WALTER B. DORSETT, of St. Louis, Mo.—In answer to the question of Dr. Macdonald, it is impossible, I suppose, to tell what proportion of physicians practising throughout the country attend either local or State medical societies; but I will say, so far as the Missouri State Medical Association is concerned, we have estimated that about one twenty-fifth of the doctors engaged in the practice of medicine in the State of Missouri attend medical societies.

DR. DUFF (closing the discussion).—I felt that in bringing a paper of this kind before the Association I would be enabled to do more good than if I had read it in my local society, because I am only an individual. It is only my opinion, and a paper read there would simply be my views on the subject; but reading it before this Association, and having it published in the TRANSACTIONS in connection with the discussions, it gives force and character to the paper which it would not otherwise have, and it will be distributed perhaps through the various journals of the country much more than it would be if it were read simply before a local society. I feel that we owe it to ourselves and to humanity to try to make an impression upon the general practitioner: I do not wish to be understood as attacking the general practitioner, because many of them are the noblest men in the profession, but it is a lamentable fact that a great many physicians are wrong on this subject. Only last week a woman was brought to me for examination. The husband told me that his wife had some little ailment, that she was having hemorrhage quite freely from the uterus, and that the family physician had told him that it was the "change of life." She was beginning to get weak, and her husband thought she had better have other advice. I found on examination that the uterus could be taken away piecemeal with my fingers; the vagina and broad ligaments were all involved. I took the husband to another room and quietly told him what the condition was, and he was astonished. He had not the least suspicion that there was anything of the kind. He condemned his attending physician, but I tried to take the part of his physician as well as I could, because most of us will make mistakes. But this thing is being done every day, and we all know it. Again, I do not want to condemn general practitioners as a whole, because very many of them are prompt in sending us their cases if they do not operate themselves. I trust the paper will bring about the results for which I am conscientiously working.

REPORT OF FOUR ADDITIONAL CASES OF UTERINE FIBROIDS COMPLICATING PREGNANCY.

By M. ROSENWASSER, M.D.,
CLEVELAND.

Uterine fibroids constitute the most serious complications of pregnancy. Previous to the advent of abdominal surgery and antiseptic midwifery more than one-half of the mothers and two-thirds of the children died.[1] The labors allowed to go to a natural termination ended as fatally as did those subjected to surgical interference.[2] Careful antisepsis and improved technique have reduced the maternal mortality to 37 per cent. in cases in which no interference occurred before labor.[3] The management of this grave complication is therefore still at fault. We must aim to obtain better results. The loss of mothers ought not to exceed 10 per cent. The viable children ought nearly all to be saved.

The number of cases recorded within the last ten years is as yet insufficient to enable us to formulate any fixed rules. Each case is a rule unto itself and must be managed in accordance with the best interests of the mother and, when possible, of the child. In placing on record these additional cases, together with abstracts of those previously reported,[4] and in submitting to your consideration their management, good or bad, the object of this paper will have been accomplished.

For convenience of discussion the report is classified under arbitrary headings, some cases belonging to two or more subdivisions.

I. Before Delivery.

(*a*) In the Interest of Mother and Child. 1. *Non-intervention.* We have all seen subperitoneal fibroids of the uterus, especially smaller tumors, that have not only not been a hinderance to delivery, but have apparently disappeared after labor. Such are not considered as complications.

We also know that in the early months of pregnancy it may not be possible to decide whether a certain tumor will become a hinderance or be safely displaced. The decision may be arrived at later on, or not until the onset of labor. Softening, compression, and retraction of apparently immovable fibroids are on record. But whenever, on account of its location, the tumor will not interfere with delivery, or when its moderate growth will admit of delay until after the viability of the child, a conservative course is clearly indicated.

CASE I. (abstract).—The patient was seen in consultation in October, 1893, when four months pregnant. She was thirty-four years old and had had three children. The tumor was of two years' growth, and occupied the smaller half of the right pelvis and iliac fossa. It did not seem likely to obstruct delivery. I advised non-interference. She was delivered at term, without difficulty, of a healthy child. Hysterectomy was performed successfully nine months later for profuse hemorrhage and intense pain.

Advice in accordance with the rule above quoted was given in three other cases. In two (Cases III. and IV.) it was not accepted, and in one (Case V.) post-partum complications developed. These three cases are therefore reported under other headings.

2. *Myomectomy.* Myomectomy for pedunculated fibroids is as safe in its results as is ovariotomy under the same conditions. The enucleation of fibroids has been followed by abortion in quite a proportion of cases ; according to recent statistics there were eight abortions in twenty-one myomectomies.[5] The indications are therefore limited. Myomectomy may be indicated after viability of the child, in preference to hysterectomy, when it is desirable and possible to preserve the uterus, or during labor when myoma of the cervix obstructs the pelvis and cannot be dislodged.

3. *Porro Operation.* After viability of the child or at term, if the tumor obstructs delivery and if the uterus must be sacrificed, the Porro operation has hitherto proved the safest procedure.

4. *Vaginal Cesarean Section.* Dührssen's[6] successful delivery of a woman at term through the cervix uteri bisected in its anteroposterior diameter has encouraged him to propose a like delivery in certain cases of myoma low down in the uterus. His advice has not yet been followed.

(*b*) IN THE INTEREST OF THE MOTHER. 1. *Induction of abortion.* If the child must be sacrificed the choice lies between

abortion and hysterectomy. To my mind the accidents attending hysterectomy can be more readily controlled than can the complications of an abortion, in which the hemorrhage may become fatal or the fibroids may slough and lead to sepsis. Often an abortion is impracticable because the cervix is beyond reach. Among the cases reported as having aborted the death-rate has been 12 per cent.[7]

2. *Hysterectomy.* There are cases in which, on account of intolerable distress from abdominal distention or pressure, or from interference with vital functions, or from constant, intense anxiety, we are obliged to do a hysterectomy as an emergency operation.

CASE II. (abstract).—The patient was seen in October, 1891. She was forty-one years old, had been married twelve years, and had never been pregnant. She had been regular until five months ago, since which time the menses had failed. She had been aware of the presence of a tumor the past two years. The recent rapid growth of the tumor and the distress from fulness of the abdomen had caused her to seek relief. The diagnosis was tumor with pregnancy. The patient thought she was entering on the menopause and would not listen to the suggestion of pregnancy. She insisted on any operation that offered speedy relief. Supravaginal amputation was made and the stump was secured by serre-neud. The tumor was a multinodular fibroid containing a fetus of about five months. The patient died septic.

Again, there are tumors located in the lower segment of the uterus, or in the anterior lip of the cervix, or in the broad ligament, which encroach on the pelvic space and constitute a barrier to safe delivery. Delay until viability of the child may be possible at an increased risk to the mother. What advice shall we give? It has been my rule to state the facts to the patient and her friends, and to let her decide whether she will accept the additional risks or whether she prefers hysterectomy regardless of present or future motherhood. In both the cases reported the decision was against delay, though neither of the women had as yet had a child.

CASE III. (abstract).—The patient was seen in December, 1895. She was thirty-seven years old, married twelve years. She knew she had a tumor the past three or four years. She had not menstruated for four months, and noticed she was rapidly growing larger. She had consulted a surgeon, whose attempt to bring on abortion had failed. The tumor filled the pelvis, extending upward to the umbilicus. The cervix was close to, but high above,

the symphysis. Diagnosis : uterine fibroid with a four-months' pregnancy. My advice to delay until after viability of the child was rejected and immediate radical operation demanded. Supravaginal hysterectomy was successfully performed. The pregnancy was in the upper part of the uterus, the tumor in the lower. The patient is now (four years later) in better health than ever.

CASE IV. (referred by Dr. W. H. Wirt, Loudonville, Ohio). Mrs. H., aged twenty-eight years ; married nine years, never pregnant ; menses regular, profuse, five to eight days. The patient is of spare build, somewhat anemic, otherwise of healthy appearance. She first consulted her physician in November, 1897, for hypogastric pain night and morning, with nausea and vomiting. There was at that time some pelvic tenderness and a bulging of the posterior uterine wall, which felt harder than a normal uterus. She improved on treatment, and was not seen again until recently.

At the time of consultation she had missed a period and was three weeks past her time, having a slight bloody discharge, pain, vomiting, and nausea, resembling the symptoms of last year. The pelvic mass is movable as a whole, has a hard posterior bulge depressing the vaginal vault, is of the size and shape of an elongated cocoanut, with distinct groove or depression at the fundus ; the left half is soft, the right is as hard as the posterior bulge and lower segment of the cervix ; the latter is slightly patulous. Diagnosis : uterine fibroid complicated by early pregnancy. After receiving an unbiased statement of her condition and prospects, the patient insisted on hysterectomy as her choice.

Operation, June 23, 1898. Suprapubic hysterectomy according to the Baer method. Recovery without incident. The softened and enlarged uterus contained numerous interstitial smaller tumors and two of the size of a hen's egg. In a niche above, near the left cornu, there was an ovum still surrounded with chorionic villi. There was no placenta. The tumors in the right and posterior segment were incorporated in the posterior uterine wall.

II. AFTER DELIVERY.

(*a*) NON-INTERVENTION. We presume that no one would unnecessarily meddle with a case after safe delivery. One is, however, liable to be called upon to check a violent post-partum hemorrhage or to deal with a septic uterus.

These sequelæ may be controlled by the means usually applied when no tumor is present, but must in some instances be promptly

and radically dealt with. In the following case, which has already been referred to above under the heading of non-intervention during pregnancy, we have exemplified both complications, hemorrhage and sepsis, the latter too profound to admit of radical measures.

CASE V. (referred by the late Dr. G. A. Orwig).—Mrs. M., aged thirty-eight years ; married sixteen years ; two children, last child two years old. Menses always profuse, one week, with bearing-down pain, but worse after birth of second child, which was weaned in November, 1897. On account of irregular hemorrhages and abnormal condition in her abdomen, I was asked to examine her in January, 1898, the attendant suspecting extra-uterine pregnancy.

The abdomen corresponded in size to a five-months' or six-months' pregnancy. Behind the symphysis pubis and toward the left side of the pelvis there was a hard tumor of the size of a fetal head. Apparently closely attached to the right of this body there was a softer, fluctuating, oval tumor extending into the right flank. There was no bulging into the vagina. Cervix soft, displaced anteriorly. Diagnosis : pregnancy complicated by a tumor. Advised non-interference.

On July 12th the patient was delivered of a healthy child at term. Within the first half-hour she was taken with a severe post-partum hemorrhage which her attendant could not control. When seen by me she was blanched and still bleeding freely. After flushing the uterus with hot water and packing with gauze, the hemorrhage ceased. The tumor proved to be an intramural fibroid, which had caused a tedious labor and afterward had prevented prompt contraction of the uterus.

A few days after confinement the patient became septic, her pulse ranging from 115 to 130 and the temperature from 101° F. to 104° F. At the end of three weeks, her condition growing constantly worse, she was sent to the hospital, with a view to surgical relief. I had not seen her in the meantime. I found her emaciated ; her pulse was small, compressible, 126 to 134 ; the temperature was 102° to 103° F. She had frequent profuse sweats ; there was a purulent vaginal discharge. I did not deem her condition at all reassuring for operative intervention, and therefore treated her as I would any other case of profound sepsis. She gradually improved. At the end of three weeks she was discharged, to complete her convalescence at home. I called on her

recently to ascertain her present state. She says she feels better now than ever. She does all her own work except washing and heavy lifting. She does not flow as profusely nor quite as long as formerly. The tumor is hard, movable, not painful, occupies the left pelvis, and reaches about two inches above the level of the pubes. There is no vaginal discharge.

(*b*) HYSTERECTOMY, indicated by: 1. *Recurrent hemorrhage.* Post-partum hemorrhage need not take place at the time of confinement, but may set in during the puerperium and may continue to recur, compelling ultimate recourse to hysterectomy. In non-septic cases this involves no more than the ordinary risk of supra-pubic amputation.

CASE VI. (referred by Dr. G. R. Feil).—Mrs. C., aged thirty-eight years ; married thirteen years ; no children ; four miscarriages at from four to six months ; menses regular, profuse, seven days. Previous health good. Was delivered of a seven months' living child four weeks previous to my seeing her in consultation. She had been taken with a severe hemorrhage, supposed to be due to retained placental remnants. Under anesthesia digital intrauterine exploration revealed the presence of a sessile, submucous fibroid on the posterior wall, about four inches in diameter. A smaller subperitoneal tumor was felt on the anterior wall. The hemorrhage was controlled by curetting and packing of the uterine cavity. The subsequent treatment, by rest and internal medication, proved of only temporary benefit, the hemorrhage recurring. The patient became extremely nervous and demanded operative relief. Operation, December 24, 1896. Suprapubic hysterectomy, Baer method. Recovery uneventful. The patient is now in excellent health. The child is alive and strong, though its development is slow. The tumor in the posterior uterine wall was a myoma about as large as a medium-sized cocoanut. There was a small fibroid in the anterior wall and one just above the bladder. The right ovary was cystic.

2. *Septic infection.* The tumor may slough or become infected, or the placenta may be retained, undergo putrefaction, and cause acute sepsis.

If the infection is local, hysterectomy is the treatment by choice. If general, one must be guided by the conditions surrounding the case.

No time ought to be lost in attempts to remove a putrid placenta from a fibroid uterus, if situated beyond the tumor and difficult of

access. The chances for life are improved by removal of the uterus, including tumor and placenta, provided the infection be still circumscribed. If the infection has gone beyond the pelvic limit my own inclination is against surgical measures, which only add the elements of shock and hemorrhage to the already debilitated condition caused by the sepsis. When we cannot tell whether or not the infection has crossed the line, the patient is entitled to the benefit of the doubt by immediate operation.

CASE VII. (referred by Dr. I. Friedman).—Mrs. P., aged thirty-two years; married six years; one child four years ago. Menses usually last ten days; they are profuse and attended by bearing-down pain. Dr. Friedman had diagnosticated uterine fibroid nine months ago and had advised operation, which was declined. Late in the fall of 1897 the patient became pregnant, and on May 3, 1898, she miscarried at five and a half months, having felt life for five or six weeks. Dr. Friedman first saw her on May 5th, and found her with a temperature of 103° F., retained placenta, distended abdomen, and rapid pulse, the os uteri admitting two fingers.

In consultation on the eve of May 6th I found a slim, pale woman with small, rapid pulse. The temperature was 104° F. The abdomen was enlarged by an indented tumor arising from within the pelvis to two inches above the umbilicus. The larger half of the tumor was on the right; it was rounded, hard, somewhat movable. The smaller was on the left; it was smooth and soft. The os admitted two fingers; the placental tissue was barely within reach. The discharge was free and quite offensive. I regarded hysterectomy as a forlorn hope, and so advised. Operation, May 7, 1898. Suprapubic hysterectomy, Baer method. Abdomen closed without flush or drain. The patient came off the table in fair condition. In the evening her pulse was 104; after midnight the pulse, though not increased, was decidedly weaker. Submammary saline injections, and finally intravenous transfusion, were made during the following day with but temporary improvement. The patient died suddenly thirty hours after operation with symptoms of heart failure. There was no autopsy. The tumor was submucous, sessile fibroid in the right posterior uterus, with a decomposing placenta in the upper part of the left uterus. The Baer operation was selected to save time.

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7. Johns Hopkins Hospital Bulletin, March, 1894, p. 33.

DISCUSSION.

Dr. John M. Duff, of Pittsburg, Pa.—This is certainly one of the important papers of this meeting. There are few subjects connected with the science and art of gynecology and obstetrics which demand better judgment than that under consideration now. We do not want to be too hasty in our work, because oftentimes, where delivery seems absolutely impossible early in labor, we will find pregnancy will go on to term, and delivery can be accomplished without any trouble. Of course, to those who have watched the subject closely, this will be rather a strong expression; but to one who has not had a great deal of experience, when he finds a fibroid tumor in the way he is very apt to feel that there will not be sufficient room for delivery of the child. It would take too long for me to rehearse the cases which have come under my observation within the past year. However, it might not be amiss to report one case. A woman had a pedunculated fibroid low down in the pelvis, with the uterus above. She was five months pregnant when I saw her. She was also seen by Dr. Sutton and Dr. King. She decided to have, if necessary, a Cesarean section or a Porro operation at the end of term. We made all arrangements to have her taken to the hospital and do either a Cesarean section or a Porro; but, to our astonishment, one night Dr. King was called to see her; she was in labor, and doing nicely, the fibroid being away up in the abdominal cavity, and she was delivered practically without any trouble. If we feel that operations of this kind are necessary, with a careful technique a great deal can be done in the removal of fibroids without endangering the life of the child. The week following our meeting in Pittsburg last year I operated upon a woman who was in the hospital at the time of the meeting. Two or three of the Fellows remember the case. A fibroid tumor had been diagnosticated, as well as a dermoid cyst. Her symptoms became so painful that it was thought best not to let her go longer without an operation. She was put on the table, with the intention of having the uterus cleaned out; but after considering the matter I concluded I would not dilate and clean out the uterus,

but go into the abdominal cavity first, which I did, removing a large dermoid cyst containing bones, milky fluid, and a large mass of white hair. The fibroid was small and sessile in character, but with a tendency to the formation of a pedicle. I had to go down into the muscular tissue of the uterus to remove the cyst. The woman went on to term, left the hospital apparently well, and, to my astonishment, about three months ago gave birth to twin daughters, perfectly healthy. Some of you will remember that I showed a picture, at the last meeting of the Association, of a colored woman upon whom I did a hysterectomy. She had several large fibroids, seven of which were the size of my two fists, all of which were enucleated during the sixth month of her pregnancy. She made an excellent recovery. She went on until the eighth month, when she was suddenly taken with a violent chill on account of a fall; her temperature ran up; she did not have much pain, except, as she said, she was sore around the loins, and died suddenly. At the post-mortem examination the uterus was found in good condition. It was scarcely possible to tell where the tumors had been removed. There was no pathologic condition to be found until we reached the kidneys, and these were full of pus. I speak of this case to bring out one particular point, especially in house-to-house practice, and that is infection through the bladder in catheterization after operation. That is frequently the cause of death. Where we fail, and our operation otherwise has been successful, it is generally from infection through the kidneys. I do not know how often this occurs, but I have had enough experience in this direction to lead me to believe that it is rather frequent, and we should be absolutely careful about the use of the catheter under all circumstances to see that there is no infection introduced into the bladder.

I thank Dr. Rosenwasser for presenting his paper, because it is very interesting, and is one which elucidates the subject very clearly, which will in the future be of very much more importance than it has been in the past, because operative measures will be resorted to more frequently hereafter than they have been heretofore.

Dr. T. J. Crofford, of Memphis, Tenn. I think this paper is a very entertaining one, and the question of just what to do in such cases always puzzles me. I recall to mind two cases, one of which occurred about three or four months ago. A patient came to me and stated that after a delivery, a few years before this, she had infection. She had a tumor for four or five years, which had been diagnosticated between pregnancies as a fibroid. She wanted to have an operation done and the organs taken out. I told her to come back in a few days, when I would examine her more thoroughly, determine the condition of the kidneys, and possibly do an operation. In the meantime she had become too weak to undergo an operation. I gave her

medicines to build her up and to get her in as good condition as possible, so that when labor set in we would likely do a Porro operation. When labor came on she had no great difficulty, and after it she escaped infection, but still had the tumor. Now she is in good condition. I do not know how the tumor is going to behave, but she is much disappointed that she is not rid of it. Possibly if I had done an operation she might have been worse.

Another case. I went over into Arkansas to see a woman who had been in labor for five or six days. She had a tumor complicating delivery. The surroundings were bad. We put her on a car and brought her to Memphis. About the time she got there the head of the child presented, and she was delivered. A few days later she had a good deal of threatened peritonitis. I opened the abdomen, expecting to do a hysterectomy, but I found diseased tubes and ovaries. There was a good deal of peritonitis, and she behaved so badly during the operation that I was afraid to resort to radical measures. I thought we would get rid of the peritonitis by removing the diseased appendages, which we did. She went on for about a month and accidentally got well. The fibroid which had interfered with delivery sloughed, infection took place, but, in spite of the miserable condition she was in, finally recovered.

These are the only two cases that I recall at this time as having a bearing upon this paper. I have had a number of miscarriages from fibroid tumors, etc. As I have previously said, it is possible that if I had seen these cases earlier and had operated, they might not have done so well. Good judgment is as essential in these cases as anywhere in the practice of medicine. Sometimes we are too eager to operate, and sometimes we are to blame when we do not operate. So I do not think the last word has been said. There will likely be a better understanding about these cases in the future than there is at the present time.

Dr. D. Tod Gilliam, of Columbus, Ohio.—In taking a bird's-eye view of this subject there are certainly indications for operative interference in a very large proportion of these cases. When we have a mortality of 50 per cent. as the result of non-interference, we know that we may improve it by operation. We know that we can get along with these cases with a much less mortality than that—at least one-half. There are several features to be considered. Of course, by very early radical operative interference a very large number of these patients can be saved, but we have got to consider the life of the child. It may not always be consistent to remove the entire uterus under these circumstances, because we want to preserve the life of the child. In a number of instances the child does not live to full term and is thrown off. Frequently we may do operative work during the period

of gestation without very greatly endangering the viability of the child, such as myomectomy. We may frequently go in and remove these fibrous nodules from the uterus and leave the uterus in a condition so that gestation may go on favorably, without interfering with the viability of the child. In such cases I think it is our duty to do so. In other instances, where we feel it is not practicable and we want to preserve the life of the child, it is better to wait until we see evidences of death of the child, and if we cannot satisfy ourselves of the death of the child, we might wait a little while, but not leave the case to nature, because nature is the cause of so many deaths of mothers. Just as soon as we see evidence of a miscarriage or anything of that kind, we should inaugurate interference, and by that means we can save many mothers without endangering the lives of the children. If the mother goes on to full term it would be economy for us to interfere and not let labor go on unless we see that everything is favorable for the expulsion of the child. There may be no impediment in the canal through which the parts are to pass, because they arrange themselves in a certain way, and nature does a great deal. The contractile power of the uterus may not be exerted in the proper direction in some of these cases, so that we will have dystocia. But even if the child is expelled, then we have to do with the secundines, and sometimes they will cause trouble. Supposing the child has been expelled, we may have to deal then with secondary hemorrhage, infection, etc. Considering one case with another, it is better to advise a patient to undergo operation either at the time of viability of the child or at the time of the commencement of labor, doing a Porro operation. This would be a life-saving affair. I have had an experience which corresponds very closely with the experiences of Drs. Duff and Crofford, in which accidentally the child has been born without trouble and the mother has suffered no inconvenience during its delivery or subsequent to labor.

Quite a while ago I was called in consultation to help deliver a woman who had been in a long, serious, and difficult labor. When I reached the house the attending physician had the forceps applied, but could not effect delivery. I delivered her and retired to another room. Pretty soon he called my attention to the fact that there was something wrong. On examination I found some large fibroids in the uterus, and advised that we do something for the woman after her getting up. But things went on nicely, and I never heard any more about the patient except that she recovered. This would admonish us that every case stands alone and should be the subject of careful and critical study.

Taking a general and broad view of the subject, I would say that operative interference should be more frequently resorted to than it is, and by that means I am sure that we can lower the death-rate to a considerable extent from what it is today.

DR. H. W. LONGYEAR, of Detroit, Mich.—Statistics do not always prove what we want them to do, and I think in these cases of tumors complicating labor the statistics of 50 per cent. mortality do not prove very much, for the reason that there are a large number of cases of fibroids complicating labor which we never hear from at all. The general practitioner does not report such cases if they turn out all right. It is only those cases which the specialist is called to see that get into medical literature. We have all seen cases that were never placed on record, where a good-sized fibroid has complicated labor, the woman has gone to full term, and has been safely delivered. These cases are so varied in character that it is a difficult matter to classify them in such a way as to lay down any rule for operation or non-interference. It seems to me, therefore, that the question simmers itself down to individual judgment in each case. A fibroid tumor which, before pregnancy, might seem to be a positive obstruction to anything that had to pass through the parturient canal, sometimes, during pregnancy, owing to the growth of the uterus and the elasticity and dilatability of it, may permit labor to go on to full term, and be pushed upward at a distance sufficient to permit the passage of the head of the child through the canal. I have seen this; many of you have seen it, and the case reported by Dr. Duff is one of them.

Ten years ago I had a case in which there was a soft myoma of the anterior uterine wall; the woman had not been pregnant for many years, but she became so. After considerable deliberation over the case I concluded there would not be any serious obstruction to labor; I allowed pregnancy to go on; she aborted at the fifth month. Labor came on quickly and violently, and before I could reach the house the fetus and placenta were all expelled. Everything seemed to be all right, but the second day thereafter violent pains occurred. Upon examination I found a fleshy mass presenting at the os. Catching hold of it with forceps and following it up with the finger, I enucleated a mass as large as a cocoanut, which was a soft, edematous fibroid.

Septic infection occurred in this case; also phlebitis in both legs. The woman had a serious time, but she recovered and is now in excellent health.

Dr. Duff's paper is a valuable one, in that it directs our attention to the dangers occurring from tumors complicating pregnancy. It is a difficult matter, however, to formulate any rules regarding it, and our actions must depend upon individual judgment.

DR. M. ROSENWASSER, of Cleveland, Ohio.—The spirit in which my paper has been discussed is in accordance with the thought I wish to convey in writing it—that is, each case must be judged by itself. I again call attention to the dangers that may arise after a woman

with fibroid has been delivered. The dystocia may be relieved at the time of labor, but our troubles are not yet ended. Cases will occur where hemorrhage, sepsis, and death follow, though the uterus has not been interfered with. This is the class of cases the Fellows have mentioned in the discussion.

As to statistics, of course, we know they are not always reliable. The statistics I have given were taken from the papers of Gusserow, Olshausen, and Stavely. I have excluded from consideration those cases which did not offer any obstacle to delivery.

The part of the paper I desired to have discussed most has not been touched upon in the discussion; it deals with a question I would like to have answered. I have reported two cases in which I made a plain statement to the patients; one was four months pregnant, the other only three weeks. They were anxious to know whether there was any danger connected with the tumor as pregnancy advanced. I replied that I could not tell whether any or how much trouble might arise at the end of delivery or afterward. I gave each patient a simple statement of the facts, giving her the *pros* and *cons* in the presence of husband and friends, and let her decide whether she would have an operation or await developments. The question I wish to submit to the Association is, Am I right in my position? Has the woman a right to decide for herself whether she shall have a radical operation at once, removing the uterus and tumor, or is it my duty to insist upon her waiting till the child is viable or until the term is up?

In the two cases I have reported, after giving both women a fair statement of their condition and the prospects of both of them going on to full term, they preferred to have an operation performed at once.

Dr. Crofford.—Since Dr. Rosenwasser has asked a question, I desire to say that the woman has a right to be something more than a procreating machine. While it is usually best to wait for labor to set in, the child ought to be considered; and yet if she should happen to be in the South, where it is very hot, and is to be delivered in July or August, when the most experienced operators are away from home, this might be best.

Her side is worthy of consideration; she should have rights in the premises. Take the question of abortion (and I am sorry I was not here when it was discussed), there are some who do not seem to consider the woman at all. Many a woman's health has been wrecked by delay; she becomes an invalid; her kidneys become diseased. I believe she has a right to take care of her health. If she has peritonitis or any serious complication; if she has pain, which is an evidence of disease; if, in the opinion of the physician, any existing form of disease will in any way jeopardize her life or her health, pregnancy should be terminated. She has a perfect right to demand an immediate

operation. She has a right to be other than a procreating machine at her peril.

DR. EDWARD J. ILL, of Newark, N. J.—I have purposely refrained, as your President, from taking part in any of the discussions until now, but I cannot permit such statements as have been made to go by without a remark. While the mother has got rights which we must respect, the child likewise has rights which must be respected. The mother understands the dangers she undergoes and has to contend with during her pregnancies and her confinements, but a certain percentage (I will not say how large) of women of the more civilized type suffer after their confinements. Now and then one becomes an invalid; we do not cure all of them. According to what has just been said, these women have a right to demand an abortion because they think they are going to become invalids. They may desire that an abortion be produced, and may ask us to do it. As I said in my address, I have never yet been obliged to destroy a child, and I have never lost a mother. This thing has been done entirely too often.

So far as the position of Dr. Rosenwasser in regard to his case is concerned, the same question was brought up in one of the New York societies, and one man hit the nail on the head when he said: "I must do it; if I don't, my neighbor, Dr. So-and-So, will." And I think there is where the fault comes in. We have not got the courage to say to these women that there is no reason why they should have their tumors interfered with at present.

DR. CROFFORD.—Perhaps a word or two of explanation is necessary from me. We know that there is quite a sentiment against operative interference in these cases of tumors complicating pregnancy. It takes a man with an immense amount of courage to face religious sentiment, and I think he needs just as much courage on that side as the other. So far as doing a thing because I think my neighbor might do it if I did not, is simply childish. The man would be a coward who would do a thing of that sort. An operation should be indicated in these cases before it is undertaken. Each case should rest upon its merits irrespective of outside sentiment.

CHOICE OF METHOD FOR TOTAL HYSTERECTOMY AND SOME POINTS OF TECHNIQUE.

By B. SHERWOOD-DUNN, M.D.,
BOSTON.

In 1895 I returned from Paris after a residence of ten years there, and having seen a great deal of vaginal hysterectomy by the clamp method in my hospital service, and being its enthusiastic admirer, I praised it as an ideal operation, easy of performance, and largely devoid of the dangers attending the operation by the abdominal route. In France it is universally accepted and generally practised, and in the hands of Professor Gustave Richelot has shown a favorable rate of mortality which has never been equalled by the abdominal operation. In this country it has, here and there, won an admirer and advocate, but has failed of popularity, and for some time I was at a loss to understand why. Fuller acquaintance with the technique and results of the abdominal operation in the hands of my American colleagues has not only enlightened my mind as to the reason, but has converted me to the American operation of abdominal hysterectomy for many, and, I may say, the majority of cases in which I formerly practised the vaginal route. The evolution and perfection of the technique of abdominal hysterectomy is distinctly American, and until the publication in Paris by Professor Paul Ségond of its details, after his visit to this country, the American operation was almost unknown in France.

My change in opinion and procedure has been slow and reluctant, and results from conviction, as, point by point, I have been led to see the manifold advantages of an open field for determining the conditions present, offering freedom for manipulation, and the better conditions for doing a complete operation. I first decided that cases complicated by firm adhesions were better handled through the abdomen; this left pus, cancer, and simple cases (in which I include small fibroids) for the vaginal route. Later my

natural desire for conservatism persuaded me that the simple cases, if operated through the abdomen, offered the possibility of preserving the central organ and possibly one of the appendages, which I look upon as of vital importance to the well-being of women of younger years. There remained then only pus and cancer cases. Now, I am fully satisfied that after the removal of diseased cervical tissue and the proper preparation of the uterine cavity, the removal of a cancerous uterus by the abdominal opening offers better promise of success, as it affords the opportunity to examine and remove any of the affected glands, as well as any suspected portion of the broad ligament.

Vaginal hysterectomy, for me, has therefore resolved itself into an operation of choice for dealing with pus cases, and this comprises the enlarged and soggy uterus of metritic or puerperal origin, with or without complication of the appendages, for the removal of which vaginal hysterectomy is an ideal operation.

The superior advantages offered by the vagina for natural drainage, together with the protection this route offers to the barrier which nature usually sets up between pus accumulations in the pelvis and the abdominal cavity, have held me a partisan still to the vaginal route for the treatment of extravasated pus tubes, perimetritic and pelvic abscesses. The criticism can truthfully be made that in the presence of these conditions in their worst forms the operator does an incomplete operation, in so far as he leaves undisturbed some adhesions which may later necessitate a second operation through the abdominal wall, and, in some instances, more or less diseased tissue which sloughs away in the drainage and prolongs the after-treatment and convalescence; but those who have had experience in the handling of these cases by both methods will, I feel confident, bear me out in the statement that vaginal hysterectomy is a life-saving operation, and the importance of this consideration outweighs all others in my mind. The foregoing reasons are equally cogent in persuading me that certain cases and conditions of pelvic abscess are best treated by vaginal incision and drainage, even if necessary to follow later with a radical operation. Given the choice, I believe any patient will elect two operations offering greater security to life than one where the outcome is a matter of *prognosis infausta.*

The advantages claimed for vaginal hysterectomy are, briefly—arranged in the degree of their importance—that the operation is attended with a less percentage of mortality than the abdominal

operation; that it occupies far less time in its performance with clamps, with consequently less shock to the patient; that it is practically never followed by hernia; that it offers natural drainage downward, which is important in pus cases, and equally permits of daily flushing and cleansing of the field of operation, which is an important factor in the elimination of pus and pockets and tends to final closure without adhesions; that it avoids the abdominal scar, which is unsightly if closed with the through-and-through suture or by interrupted superficial sutures of silkworm-gut, and is a source of annoyance to some women, and in a certain proportion of cases gives rise to hernia.

The disadvantages are—danger to the ureters, which vary in their anatomical relations; danger to the intestine and bladder; difficulty in the ready control of all oozing, which is a vitally essential point in the technique; the great pain suffered by the patient during the forty-eight hours that the clamps remain, and the time and trouble necessary to the proper daily dressing of the patient over a period of from fourteen days to six weeks, depending upon the conditions present.

The importance of the after-treatment of these cases has been generally overlooked and misunderstood by American surgeons, and, as regards the ultimate recovery and favorable results, this is as important as is the technique of the operation itself. The reason for this oversight is easily explained. The clinics are open to all without question in Paris, but a visitor, to follow the cases in the wards and daily see the dressings, must obtain the consent of the hospital administration, which is difficult and is seldom accorded. I have seen a host of my American colleagues present at the operations in my hospital service in Paris, but only rarely one that had sufficient interest to secure the entrance into the hospital wards, come daily at 7.30 in the morning, and go through the day's routine. They all became familiar with the technique of the operation, but very few learned the details of the after-dressing of these cases. No case, in my opinion, can be properly douched and dressed on a bed-pan in bed. The patient should each time be placed on the operating table in the operating-room, or dressing-room if one be provided, and with the advantage of an open field brought into view by retractors in the hands of an assistant, with a good light and perfect asepsis, thoroughly and carefully douched with permanganate, hydrogen dioxide, or other antiseptic, and redressed with drainage of iodoform gauze, and the vagina carefully

but not too tightly packed with the same material. It will readily be seen that this is a laborious and lengthy operation, which, so far as I am concerned, cannot be entrusted to the hands of a nurse, no matter how competent, and must be confessed has had great influence in my relegating the operation to the saving of life only.

This brings us to the consideration of a point of cardinal import in a choice between the two methods. There is in every case of vaginal hysterectomy by clamps a sloughing of the necrosed tissue, resulting from the pressure of the clamps; and if the operation is in the presence of purulent conditions, the discharge is likely to be increased. Thus, one cannot escape the reflection that, if the mortality rates in surgery are more largely dependent upon aseptic conditions than upon any other one factor, then it is certainly necessary to exclude from our hospitals septic cases that are a constant menace to the other inmates.

In controversion of the opinion I have heard generally expressed, I believe that vaginal hysterectomy is a more difficult operation than abdominal hysterectomy, requiring a more delicate touch and manipulation, intimate knowledge of all the hidden parts involved, which is only acquired by experience in the operation, and that the successful treatment of conditions encountered, which are never twice the same, demands greater dexterity and what is termed surgical sense in its best definition. Moreover, you are likely to have complications in or following this operation when least expecting them. In my last case, which was non-adherent, I had a fecal fistula resulting from rubbing of the bowel against the point of one of my forceps, causing a necrosis which opened on the fifth day, closing, however, in due time of its own accord, as the majority of these fistulæ do.

In my radical change of opinion as to the field of usefulness of vaginal hysterectomy, I find myself in the distinguished company of some of its best known teachers and former advocates. Professor Paul Ségond, returning to France after his late visit to this country, read a paper before the Paris Surgical Society, and published in the *Revue de Gynécologie*, entitled "Abdominal Hysterectomy for Larger Fibroids, and the Superiority of the American Method over All Others." Jacobs, of Brussels, came to this country on a missionary tour to convert American surgeons to vaginal hysterectomy; he returned a convert to our suprapubic operation, and recently told an American surgeon visiting his clinic that

he considered it the operation of choice for the total extirpation of the uterus and its appendages.

The American operation has the great advantage of simplicity over the methods called after the names of Richelot, Doyen, and Le Bec, of Paris, and Martin, of Berlin, which operations are the best known in Europe.

The Richelot operation consists in the liberation of the cervix from its vaginal insertions through the abdominal opening, commencing by the dissection off of the bladder, seizing the vaginal portion of the cervix, after complete liberation, and drawing it up and forward into the abdominal cavity, and then progressively cutting and tying the broad ligaments from below upward, and finally delivering the uterus and adnexa entire. The opening into the vagina and the cut borders of the broad ligaments are then approximated and closed by interrupted catgut sutures, instead of the simpler and more quickly accomplished continuous suture of the American method.

Doyen's method resembles that of Richelot, except that he commences on the posterior aspect of the cervix, opening into the posterior vaginal fornix, and draws the cervix backward and upward into the abdomen. This maneuver is exceedingly awkward and difficult when you have a short vagina and a deep, narrow pelvis.

Le Bec's method, used chiefly for fibroids, is a compromise on the combined method, so-called, of vagina and abdomen. He commences through the abdomen by ligation of the ovarian vessels, at both the inner and outer sides of the ovary, through an opening made in the transparent portion of the broad ligament; then, after dissection off of the bladder in front, goes again through the opening in and behind the broad ligament, and carries his ligature through into the vagina and back again over the anterior surface of the broad ligament, and ligates the uterine arteries on each side. Commencing from above, he cuts the broad ligament down to his ligature on the uterine artery, both sides, amputates the uterus at this point, and dissects out the cervix, leaving sufficient tissue to form a stump on each side to prevent the slipping of his ligature on the artery, and draws these stumps down into the vagina by the aid of the loose ends of his ligatures, left long for this purpose, sutures the peritoneal surfaces together from the abdominal side, in uncomplicated cases, shutting the stumps, with their ligatures, into the vagina.

Martin's method resembles that of Doyen, with the difference that he commences by the liberation of the uterus from above down-

ward to and including the uterine artery ; then by the vagina he forces a long pair of pointed forceps through the posterior cul-de-sac into the peritoneal cavity, enlarging this opening by lateral incisions, thus freeing the uterus from its posterior vaginal attachments. He then stitches the border of the vaginal incision to that of the peritoneum with interrupted catgut sutures, knotted on the peritoneal side, with the ends left long, which arrests the hemorrhage from the posterior and lateral recurrent arteries; he then carries his first incision around the cervix in front to the opposite side, frees the bladder, completes the anterior opening into the vagina, and delivers the uterus. He then carries the long ends of the interrupted sutures on the posterior and anterior incisions down into the vagina, closes the peritoneum with continuous catgut sutures from the abdominal side, and from the vaginal side sutures the vaginal mucosa together.

This brief description of the leading European operations is sufficient to show the greater perfection and simplicity of technique that marks the American operation, the details of which are too well known to bear repetition here before this distinguished society. So far as my observation has gone, the leading operators seem pretty well united in their methods of procedure in the abdominal operation, excepting possibly more or less pronounced difference of opinion, still existing, as to the indications for doing a total extirpation or for leaving the cervical stump.

I believe there are a number of reasons why the cervix should be conserved in this operation :

1. In its removal you always encounter a smart hemorrhage from the recurrent lateral and posterior vaginal arteries, which to properly secure and close, in the closure of the opening into the vagina, prolongs the operation from some moments to a half-hour, according as the pelvis is deep and narrow or the abdominal walls thick and rigid.

2. The liberation of the vaginal insertion to the cervix demands that the operator shall keep in close proximity to, and even encroach upon, the cervical tissue to avoid injury to the ureters, and a slip or misstep may cause irreparable mischief.

3. In many women the os and cervix is the principal seat of sexual pleasure, and its removal may seriously affect the future marital relation.

4. I believe the cervix is an integral part of the pelvic floor and necessary to the preservation of its normal functions.

5. The opening from the peritoneal cavity into the vagina increases the chances of infection, no matter how carefully the vaginal disinfection may have been prior to the operation.

It may be claimed that the leaving of the stump endangers the peritoneum anyway through the cervical canal; but I have found that if in its amputation the incisions are made in a decided V-shape, apex down, and the canal thoroughly sterilized by drawing a strip of gauze through it from above downward, soaked in pure dioxide of hydrogen or bichloride, and then the two V-shaped flaps firmly stitched together and the peritoneal borders stitched over above, this danger is obviated. All the steps of the operation are greatly facilitated by the Trendelenburg position.

Where hysterectomy is done for malignant disease the whole of the organ must, of course, be sacrificed; and there are cases of tubercular disease and gonorrheal infection, where the endometrium entire is suspected, in which it may be unwise to leave the cervix; also, if the liberation of extensive adhesions or the removal of a widely adherent fibroid leaves raw, oozing surfaces which cannot be covered and must be drained, the vaginal drainage may be more important than the conservation of the cervix; but so far, I must confess, I am strongly inclined to preserve the cervix in every instance possible.

There are a number of highly important advantages which seem to me destined to convert the partisans of vaginal hysterectomy to the abdominal route and the American operation, and which will give to this operation a fixed and permanent place in gynecic surgery. Prominent among them is the opportunity to see the greater part of what you are doing, and equally to find and determine all of the pathology present, making accurate diagnosis possible, which, when determined, renders a complete operation possible; greater ease in overcoming unforeseen complications and in repairing injuries to the intestine, which are less likely, however, to occur in this operation. The danger of injury to the ureters is decidedly less, and, with all of the vessels under your eye, danger from hemorrhage is lessened. Last, but not least, the stitching of the cut borders of the broad ligament and cervical stump, and the covering of them by peritoneum, protect the intestine from subsequent adhesions and lessen the danger from volvulus.

COCCYGEAL DERMOID FISTULA.

By ROBERT T. MORRIS, M.D.,
NEW YORK.

The subject of coccygeal dermoid fistula is apparently one that has not received very much attention, as I find very little reference to it in the literature. Occasionally some one has reported the finding of a dermoid cyst in the coccygeal region, but without making satisfactory explanation for the derivation or manner in which such cysts are likely to have been formed. I have seen reference to cysts only, but in making a study of the subject I find that we are much more apt to find fistulæ than cysts. We are more apt to find funnel-formed depressions than fistulæ. We have, then, in the region of the coccyx not infrequently funnel-shaped depressions of the skin. We have less frequently fistulæ extending to the depth of from half an inch to four inches, as in my deepest case, and we have still less frequently wholly encapsulated cysts. These fistulæ and cysts contain straight hairs of the lanugo type mostly, although I have seen some nearly three inches in length, straight, in a mass, and welded together by sebaceous material. The sebaceous material escapes in large quantities from the fistulæ; it remains encapsulated in the cysts, and in the funnel-shaped depressions it frequently dries upon the surface and comes away in the form of scales mixed with epidermis.

When my attention was directed to the subject, about a year ago, I was surprised at finding so large a proportion of cases in which we have these funnel-formed depressions or fistulæ, or cysts of the coccygeal region, or dermoid cysts. It evidently represents an embryonal defect, and what this defect is I leave for the members of this Association to suggest. There is a possibility that the tail of the embryo in undergoing involution leaves a portion of the skin highly endowed with embryonal latent cells, and that the skin,

developing more perfectly in the tissues about it, encapsulates in part or in whole this absorbing or involuting embryonal tail, so that we have in coccygeal fistulæ really a tail the wrong side out—an inverted tail.

I have had four cases of coccygeal fistulæ containing masses of hairs in my own practice up to the present time. In trying to eradicate them we must remove the entire cyst wall or fistula wall, because it consists so largely of embryonic tissue that we shall have recurrence following the operation unless it is all removed. The method of treatment with nitrate of silver and other caustics will result only in temporary destruction of the inner layer, just as when we try to destroy branchial cysts or branchial fistulæ. We have a recurrence very promptly from the deeper layers of the embryonal cells, the latent cells forming the structure again, so that it is very important to be thorough in their removal.

The patient from whom I show these slides under the microscope was a young man, twenty-three years of age. He had not been aware of anything wrong in the coccygeal region until about two years previous to the time I saw him, when he was kicked, and following the kick there had developed a small tumor-like mass in the coccygeal region. His physician brought him to me on the supposition that injury had been done to the bone, and it was a case of necrosis of the coccyx; but I recognized the disagreeable odor of the sebaceous fistulous secretion, and found two or three hairs projecting. I dissected out the fistulous tract, which was about three inches in length. The coccyx in some of these cases seems to be deformed, but I am not sure that this is an accurate observation, because I have not had an opportunity to examine post-mortem any coccyx in which we had coccygeal fistulæ, cysts, or funnel-shaped depressions. I presume that this young man two or three years ago had a coccygeal cyst, and the epithelium at the opening, which became the fistula afterward, was originally agglutinated, very much as it is in preputial adhesions; ill-developed epithelium, not well stratified, and the cells intermingling in such a way that there is not a clean cleavage. He had received a blow in this region; there was such an increase of serous elements that they, mixing with the sebaceous elements, escaped by the lines of least resistance through the fistulous tract that had been occluded during the twenty-one years of his life.

On making a microscopical examination of the wall of this fistula I find structures demonstrated in this specimen, removed about an inch and a half from the external orifice, which comprised almost typical skin in structure, but containing an enormous number of bloodvessels, with extensive infiltration of cells like leucocytes. The epithelial surface is arranged rather irregularly, and the hairs are shed almost as rapidly as they become fully developed, so that only a few hairs remain firmly attached to the skin within the fistula. They are shed in such a way that they remain longitudinally compressed against each other, and one mass that I removed was as large around as my thumb and about as long. In most of the cases we have a disagreeable odor. I remember now that patients have sometimes spoken about moisture in the coccygeal region and a disagreeable odor occasionally, and I have glanced at the part without paying much attention to it, when, if I had used a fine probe, I might have found a fistula an inch or two inches deep.

This specimen, which is now mounted for examination by the members, was taken from a part of the wall of the fistula. You will notice the large number of bloodvessels I have referred to. The specimen shows pretty well the typical arrangement of the wall of the fistula.

What I desire particularly is some suggestion that will lead me to understand the meaning and character of the embryonal defect here represented.

DISCUSSION.

Dr. D. Tod Gilliam, of Columbus, Ohio.—Mr. President: While the Fellows are looking at the specimen I desire to say a few words. I am very much obliged to Dr. Morris for his remarks in connection with this subject, for the reason that I think he has thrown considerable light on a case I had some years ago, of which I had a vague idea as to the nature of the trouble. The case was that of a young lady, fifteen years of age, who was brought to me on account of a persistent, disagreeable discharge from the region of the coccyx. I remember one thing particularly, and that was the disagreeable odor to which Dr. Morris has referred. I slit up the fistula, removed quite a quantity of tissue, but I do not recall seeing any hairs or sebaceous matter. I was

not looking for those things. There was one thing that impressed me particularly—namely, the canalization of the tissues. As I have said, I removed quite a quantity of tissue, and closed the opening, hoping the girl would be all right. After a time she returned in as bad or worse condition than she was at first. I made another dissection, and still she returned after the lapse of a certain time. Finally, the case fell into the hands of my son, who made several operations. His dissections were quite extensive. I do not know whether the case is well now or not. This was possibly a case of coccygeal fistula unrecognized.

ONE FORM OF OVARIAN DISEASE NOT GENERALLY RECOGNIZED: PRIMARY SCLEROSIS OF THE OVARY.

By WILLIAM H. HUMISTON, M.D.,
CLEVELAND.

In the various discussions to which I have listened on the subject of chronic ovaritis, I have felt that few men fully appreciate the pathology of chronic inflammation of the ovary. For a number of years past I have been careful to differentiate, both clinically and microscopically, between a chronic ovaritis resulting in a sclerosed organ, and an ovary whose sclerotic condition cannot be, so far as the clinical history and the microscopical examination can determine, the result of a previous cell infiltration, etc., or what we ordinarily term inflammation.

J. A. Shaw-Mackenzie, an English pathologist, said in 1881: "I cannot help thinking, of all the terms applied to inflammation of the ovary, cirrhosis is the least desirable," and "I do not think we can take cirrhosis of the kidney as our guide with its pre-cell infiltration and ultimate fibrotic condition." Yet I find a few authors who recognize a non-inflammatory cirrhosis of the kidney, which they call chronic interstitial nephritis; and another variety, in which the microscopical appearance gives evidence of inflammatory changes, they term a *subacute* interstitial nephritis, *merely for the purpose of differentiation.*

One is a primary or non-inflammatory cirrhosis, and should not be termed a nephritis; the other is but the natural result of inflammatory action. And so I find in the routine examination of ovaries that there are two separate and distinct pathologic conditions which can readily be determined by the microscope: the one a primary or non-inflammatory, and the other a secondary or inflammatory sclerosis.

The ovaries upon which Shaw-Mackenzie gave his report were in every instance implicated in a general pelvic inflammation, and were, of course, of the secondary or inflammatory type, and I agree with him in saying that cirrhosis or sclerosis is a poor term to apply to a chronic inflammatory change in the ovary.

I need not occupy any time in describing the gross appearance of these diseased conditions, but will call attention to two points: first, in a *simple* case of sclerosis there are never any signs about the tube and the ovary indicative of past inflammatory action; and, second, there is not the marked contraction in a chronic ovaritis as in a non-inflammatory sclerosis, and this again corresponds to the gross appearance of the kidney in the so-called interstitial nephritis (or small granular contracted kidney) and the *subacute* interstitial nephritis.

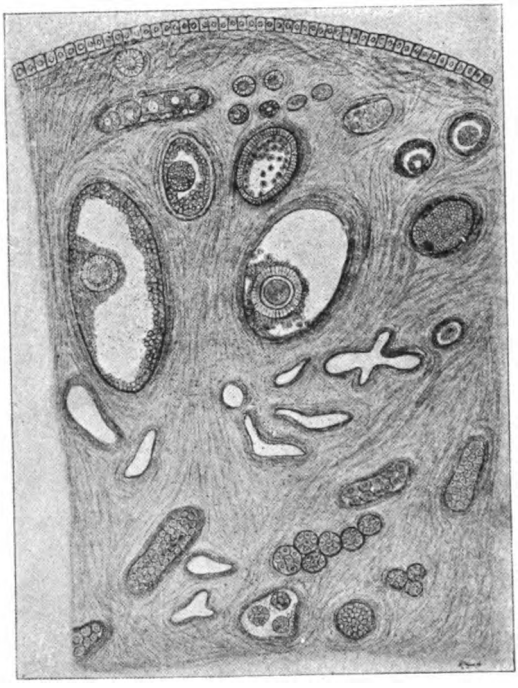
The microscopic differences between the primary (non-inflammatory) and the secondary (inflammatory) degenerations are especially quite marked.

In the secondary degenerations we invariably find in a section taken from some portion of the ovary a small round-celled infiltration, or the connective tissue elements in some of their various stages of development. Also, however advanced the stage of fibrosis may be, the numerical relation of nuclear to cellular elements in this variety is always greater than in the primary form. And, thirdly, the microscopic appearances of the vascular system are also different.

In the inflammatory group we have primarily a dilatation of the vessels with secondary contractions of their greatly thickened walls and a lessening of their lumen.

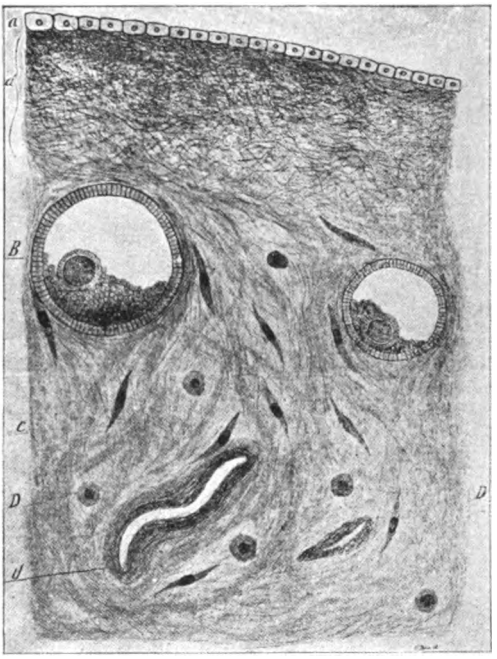
In the primary group there is usually no thickening of the vessel walls, there is never any dilatation, and contraction occurs so early that you immediately remark the poor blood-supply, particularly to the periphery of the organ. In those cases of arteriosclerosis of the vessels within the broad ligament, we usually find the condition extending to the vessels entering the ovary, not otherwise. And, lastly, in the primary group the epithelial cells of the ovarian stroma are diminished in number and usually in size. In some sections they are hard to distinguish from the connective tissue elements and appear to be undergoing a process of reversion. I would not state this as a fact as yet, but it does frequently so appear.

FIG. 1.



Normal ovary.

FIG. 2.



Primary or non-inflammatory sclerosis (diagrammatic). a, germinal layer somewhat flattened and with small nuclei; a′, thickened fibrous tunica; B, follicle which fails to break through the tunica; c, absence of round-celled infiltration—only a few connective-tissue cells with increase of fibrous elements in the stroma; D, a few scattered epithelial cells of the stroma; U, vessel wall slightly thickened.

These several microscopic differences are so constant and so decidedly marked that I have no hesitation in separating them into two distinct pathologic groups.

Just why in these cases the *natural processes* of involution begin at a time when the functional life of the ovary should be at its high tide, and when the ovarian stroma should be thickly embedded with the germ cells in all of their stages of development, who can tell? I had a number of these cases in succession a few years ago, each showing marked sclerosis of the ovarian arteries, and I thought I had solved the problem. This, however, is not a constant factor, not even in a majority of cases sufficient to warrant any such deduction.

I do see, however, in every instance (as my records clearly show) some change in some portion of the body, either one or more arteries undergoing atheromatous change, or, quite as frequently, the evidence of interstitial changes in the kidneys. Osler says that entire families sometimes show a tendency to early arterio sclerosis, and so, too, I have found repeatedly this condition occurring in sisters; and, as Osler aptly remarks, the only explanation that can be given is that in the make-up of the machine bad material was used.

Nothing further can be said of the etiology.

Four years ago a young girl, aged sixteen years, came to my clinic. She was well developed, had first menstruated when she was fifteen years old, and had seen a slight show but twice during the following year. Instead of making an examination of the pelvic organs at the first interview, I prescribed an ordinary chalybeate mixture. I changed my medication from time to time, without, at any time during the course of treatment, marking much improvement in her general anemic condition; and although the menses did appear irregularly for a number of times, they were accompanied always with severe dysmenorrhea. Within a year—that is, before she was seventeen years old—I examined her *per rectum*, and readily found an unusually small hardened ovary, together with one much larger and firmer than normal, in the cul-de-sac of Douglas.

Just recently I examined a young Slavish woman who was twenty-one years old. She was married at twenty years of age. Menses were scanty but regular during the first six months, but

during the latter six of her married life (when I saw her) she had had amenorrhea. Examination revealed two small, corrugated ovaries, not larger than hazelnuts, lying low in the cul-de-sac.

These two cases show the possibility of the early age in which aggravated pathologic conditions of the non-inflammatory type of sclerosis can occur. Ordinarily the age of the patients whom I see varies from twenty-four to thirty years.

While the pathology of these primary sclerotic conditions is of much interest and of great importance, I desire more especially to direct your attention to the symptomatology and the diagnosis of this class of cases. So varied and so complex are the symptoms that it will be best to discuss them under the various organs which are found to be usually implicated.

The kidneys. The amount of urine passed daily is above the usual quantity of the average gynecologic case, but is of low specific gravity, light in color, and of slight acidity. If the case is of long standing there may be a trace of albumin in an occasional specimen. Very few hyaline casts, more often none whatever, are discovered by the microscope, but an increased number of the renal epithelial cells are constant.

The arteries. Excepting in the very young, some artery may be found whose walls are distinctly thickened. The arterial tension is often increased. If not obvious to palpation of the arteries, it can be demonstrated by the accentuation of the second aortic sound.

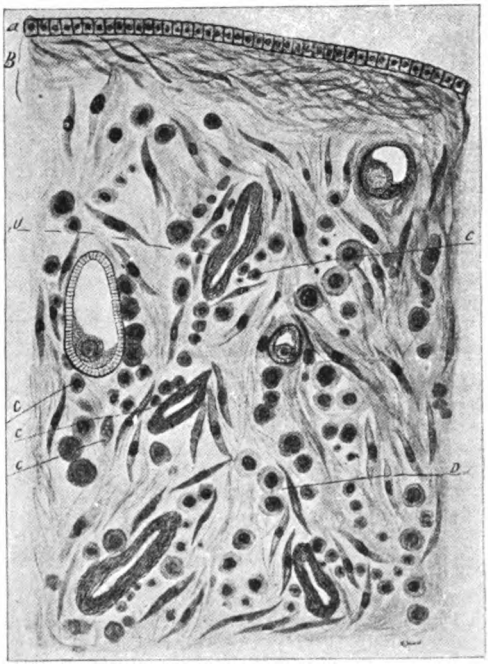
The stomach and intestines. Indigestion and loss of appetite is the rule. Fermentation with the evolution of large quantities of gas gives rise to great pain in the epigastrium and palpitation of the heart with a sense of suffocation. Chronic obstipation with occasional attacks of diarrhea are common.

The liver. The inactivity of the liver is noted in the sallowness of the patient, its sluggishness by the pain or tenderness induced during palpation, and by the color and odor of the stools.

The *nervous system* especially feels the effects of the lowered vitality of the whole economy. The patients become easily fatigued, are irritable and sleepless, and soon neurasthenia supervenes, followed shortly by one of the various neuroses.

Of the symptoms relative to the sexual organs, dysmenorrhea, amenorrhea, or delayed catamenia, constant distress with par-

FIG. 3.



Secondary or inflammatory sclerosis (diagrammatic). a, germinal layer usually absent and replaced by round cells or connective tissue; B, tunica not much thickened nor fibrous; c, round-celled infiltration with increased nuclear elements as compared with the primary form; few fibrous elements, also; D, epithelial cells of the stroma; U, thickened vessel wall and seen near the surface.

oxysmal intermenstrual dysmenorrheic pains are always present. The latter is a most important symptom. All patients describe this pain as being similar in character, and usually in intensity, to the pain accompanying the catamenia (I can only ascribe it to the effort of a mature follicle to make its way through the ovarian cortex), and is the best evidence that the dysmenorrhea is *ovarian* and *not uterine* in origin.

Diagnosis. The diagnosis of sclerosis or sclerocystic degeneration of the ovaries ought *not* to be made upon the *extirpated organ.* I should refuse to attempt the removal of an organ in any case whose appendages I could not palpate and therefrom determine their condition before operation. The history, however plain, should not be the sole basis for diagnosis and treatment, and I question the advisability of taking a history with that end in view. As in the young Slav, of whom I have already made mention, the diagnosis was made upon palpation of the diseased organs and her history obtained afterward through an interpreter.

The diseased appendage is invariably within easy reach of the examining fingers. The sclerocystic ovary is found to be *globular* in shape and very firm and tense. The small sclerosed ovary a very novice in palpation can discern, and one with a practised touch can, in most cases, feel the corrugations, which I have said before are much more marked in this, the primary or non-inflammatory sclerosis, than in the fibrosis due to inflammatory change.

In brief, sclerosis of the ovary is without a history of infection and is non-inflammatory. It occurs in early life (from puberty to thirty). Amenorrhea, or delayed catamenia, with dysmenorrhea often since the establishment of the catamenia, and intermenstrual dysmenorrheic pains always, are constant.

Evidences in not a few cases of a granular and contracted kidney, or some change in the arterial walls, or high arterial tension marked by an accentuated aortic second sound, are found. No evidence of peritonitis. Ovaries (one or both) prolapsed, firm, unyielding, and globular in form, or small and exceedingly hard and corrugated.

Neurasthenia develops early, but is quickly followed by one of the various neuroses.

Treatment. For primary or non-inflammatory sclerosis of the ovary, mercury and the iodides, together with general tonics, have

been tried. Local application of poultices and blisters to the abdominal walls in the iliac regions, the painting of the vaginal vaults with iodine, faradization and galvanization to the cavity of the uterus, together with all sorts of almost indescribable methods, have proven of no avail. There is no relief afforded by supporting the prolapsed appendages by the use of tampons or pessaries. I have administered the desiccated ovary without obtaining the slightest effect upon the symptoms and no effect whatever upon the condition. A great deal of stress is being placed to-day (especially by Continental writers) upon the internal secretion of the ovary and its effect upon the metabolism. I am as yet incredulous. I know of no way to relieve this class of patients except by castration.

TECHNIQUE OF ABDOMINAL HYSTERECTOMY.

By J. H. CARSTENS, M.D.,
DETROIT.

MANY plans of operation have been suggested and practised which are matters of history, and before an audience of this kind it is not necessary for me to take up your valuable time and review all the different methods which have been in vogue. The long dispute between the extraperitoneal and intraperitoneal treatment of the stump has, it seems to me, been decided in the last five years in favor of the latter, but this again is subjected to variations. Some advocate total extirpation of the uterus, including the cervix, while many advocate leaving the cervix in. I have always favored both operations, according to the case.

In case of fibroids, complicated or not with tubal disease, where the cervix and mucous membrane were perfectly healthy, I have always been in favor of leaving the cervix: First, because the operation can be performed quicker and easier; second, there is a better pelvic floor; third, there is a better vagina and less tendency to senile atrophy; fourth, the operation is less dangerous, perhaps with less tendency to septic infection.

When, however, the mucous membrane of the cervix is diseased, or there is laceration of the cervix, or if the case is malignant, or there is any suspicion of malignancy, I think that the total removal of the uterus, including the cervix, will give us the most permanent results. I have seen many cases, some operated upon by myself, after the old extraperitoneal method with the clamp, which were followed by discharges from the cervix, causing great distress, and in some cases I have been obliged to remove the cervix to effect a cure. I would like to make this point very strong, that in diseased conditions of the cervix the rule should be total hysterectomy.

My technique, in short, is as follows: The patient is prepared in the usual aseptic manner, especial care being given to the vagina,

because we frequently are obliged to introduce instruments and our fingers into the vagina to facilitate the operation. With the patient in the Trendelenburg position, I open the abdomen in the usual manner. The intestines are kept in place by abdominal towels. A corkscrew is introduced into the uterus and the latter pulled out of the abdominal cavity. Two large, strong clamps are now placed on each of the broad ligaments outside of the ovaries, including the round ligament, and down as far as I can get them to the cervix. Between the clamps and the uterus the broad ligament is now severed about one centimetre from the clamps down as far as the latter extend on each side (Fig. 1). With a knife an incision is then made across the uterus (a little above the attachment of the bladder) through the peritoneum. The bladder can now be easily separated from the uterus and kept out of harm's way by a retractor or abdominal towels.

Two other clamps are now placed on each broad ligament, from the places where the other clamps stop, downward to about the cervix. These clamps control the uterine arteries on each side. By again cutting between the clamps and the uterus the latter is separated almost entirely. The slight attachment in the rear of the vagina and anteriorly is now easily severed and the whole uterus, including the cervix, removed (Fig. 2).

I now have four clamps containing all the bloodvessels, which I pick up one after the other and ligate. When the bloodvessels —that is, the two ovarian arteries and two uterine arteries, and, if necessary, the two arteries of the round ligament—are firmly tied, I then take off the clamps. I may find a few oozing points which may require a separate ligature, but, as a rule, do not. I then pull the peritoneum over the stump, and with a running catgut ligature, beginning at the side, cover it smoothly down to the vagina. The other side is prepared in a similar manner. If there is much oozing from the edge of the vagina, I sometimes place a running suture from one side to the other in order to control the little bleeding, but this is seldom necessary. I sometimes leave a small opening in the vagina (about one centimetre), through which I put a small piece of gauze, a kind of safety gauze. I hardly think that it is necessary always, and it may be better not to do it, but I think it removes the fluid from the abdominal cavity, and convalescence is quicker. I leave it in from three to ten days. The abdominal incision is closed in the usual manner, in layers, with dry sterilized catgut; and let me say that I now use exclu-

Fig. 1.

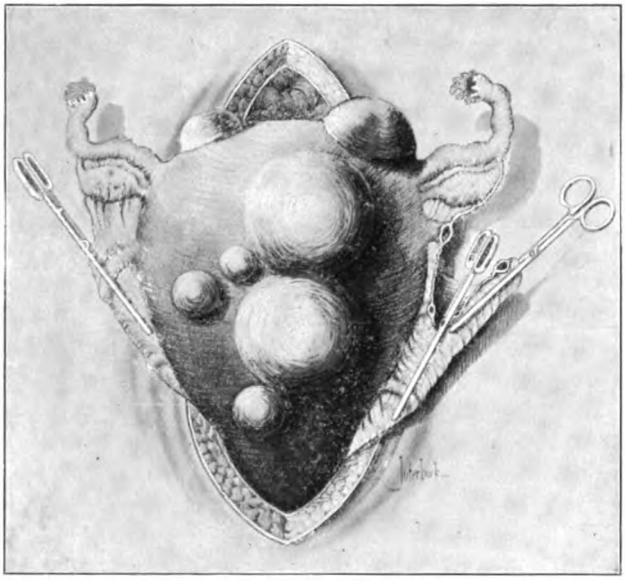
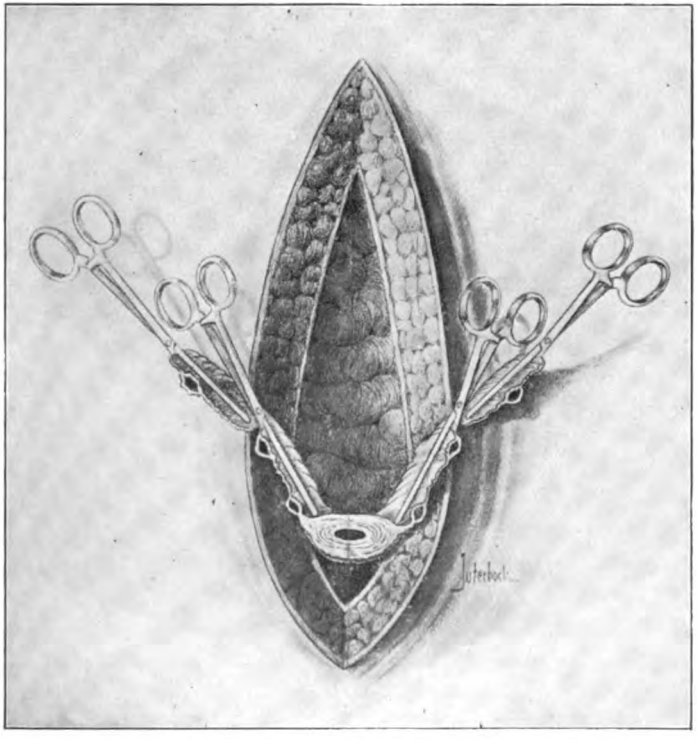


Fig. 2.

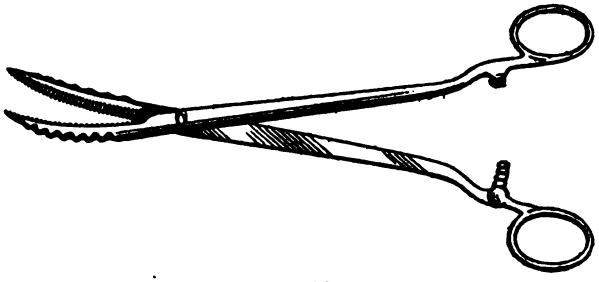


Technique of abdominal hysterectomy.

sively the dry sterilized catgut—No. 3, to tie pedicles and sew up the abdominal wound; No. 1, for intestinal surgery and to sew up the peritoneum only. I use this in all aseptic cases. In cases complicated with pus-tubes, tubercular condition, malignant growths, or any other septic conditions, I do not sew up the abdomen with catgut, but use the figure-of-eight silkworm-gut suture.

If I want to leave in the cervix I proceed as follows: After having placed the first pair of clamps on each side and made my incision as far as the first clamps extend downward, I put another pair of clamps on, but not down as far as in the first case. I also cut between the clamps and the uterus down to about the external os on each side, cutting the peritoneum from one side to the other, separating the bladder, and then making a V-shaped incision in

FIG. 3.



Puncture-dilator.

the uterus. With the anterior and posterior flap just below the internal os, the uterus is now separated in a moment. As a rule the uterine arteries are not cut, but the anastomotic branches are, and these require ligaturing as well as the two ovarian arteries, and, for safety's sake, the arteries of the round ligament are also ligated. With a running catgut ligature I bring the two flaps of the cervix together and cover this and the stumps of the arteries with peritoneum.

If there is a pus-tube and other septic conditions which I think advisable to drain, I puncture with this instrument—Fig. 3, which I have devised for that purpose—the posterior cul-de-sac. By spreading the instrument I can make the opening as large as I want to and pull a rubber tube or piece of gauze through for the purpose of drainage. I prefer not to drain. Drainage is not ideal surgery, but I am positive that in many cases drainage will

save life and shorten the period of convalescence, and I had rather have a live patient than ideal surgery.

This operation with a little practice can be so quickly performed that the shock of abdominal hysterectomy is no more than in simple ovarian tumor. Within five minutes from the beginning of the operation the tumors and the uterus are removed; the ligation of four vessels, covering over with peritoneum, the introduction of the drainage-tube if necessary, and the closing of the abdomen, can be easily done in fifteen minutes more. Of course, very complicated cases with extensive adhesions require a longer time, and I would not want to be quoted as saying that every case of abdominal hysterectomy could be finished in twenty minutes, but nearly all can in half an hour.

It is hardly necessary to say that in complicated cases the adhesions must be loosened first, or if a fibroid is in the broad ligament it must be enucleated before the clamps are applied. The angiotribe may take the place of ligatures, but we need more experience with it before we can decide its real place in abdominal surgery.

CONCLUSIONS. 1. In abdominal hysterectomy, clamp the broad ligaments and remove the growth and uterus.

2. Ligate the four bloodvessels separately.

3. Carefully cover all raw surfaces with peritoneum.

4. In cases without tears and healthy mucous membrane, leave in the cervix.

5. In any diseased condition of the cervix and malignant growth, perform total hysterectomy.

TREATMENT OF INERTIA AND SUBINVOLUTION.

By CHARLES STOVER, M.D.,
AMSTERDAM.

I VENTURE to present this topic because in obstetric practice it seems to have been neglected and too often left to the gynecologist for its detection after the puerperal period has passed and when pathologic sequelæ have supervened. Almost every physician in general practice is an obstetrician, though not necessarily an obstetric specialist. It is an ordinary custom in many localities to stipulate that two or three visits only shall be included in the obstetrical attendance. The result of this is that, in a very large proportion of cases, no attempt is made to determine whether involution has taken place, except when the woman observes something conspicuously wrong and sends for the doctor. In a theoretical sense it is true that labor is a physiologic process, but, as a matter of fact, in practice it is very far from it. Blind reliance upon nature's kindly offices leads often to dire disaster. The physician should realize that the opportunity for securing a firm contraction of the uterus is presented at every stage of the puerperium, and that it is his duty to secure this, even though his attendance continues over a period of twelve weeks.

Briefly, we may recall the fact that at the termination of pregnancy the uterus has had its depth prolonged from the normal two and a half inches to six or more; that its weight, instead of being about a half-ounce, has increased to twenty or more ounces; that its capacity has been multiplied five hundred and nineteen times, but that the walls bounding this enlarged cavity are very much thinner than in the virgin state. Retrogressive changes are now necessary for the normal restoration or involution of the uterus. Uterine muscles are to be removed by fatty degeneration, blood-vessels and sinuses are to be contracted or destroyed, lymphatic spaces are still to be maintained, and the mucous membrane regene-

rated. Leopold has shown that this regeneration of the mucous membrane requires about ten weeks for its completion. The serous membrane and the connective tissue cells must also undergo this transformation. Involution is, therefore, a continuous process of decay and regeneration, of elimination and assimilation, of retrogression and progression.

Practically all the causes that interfere with the normal involution of the uterus may be included under two heads: First, and principally, failure of uterine muscular contraction; secondly, and somewhat dependent upon the first, interference with the normal circulation of the blood.

When these obstructive conditions are exerted we have inertia and subinvolution—the first during the progress of labor, the latter after its completion. They are not, strictly speaking, pathologic processes, but rather states of physiologic arrest. They should be distinguished from a metritis or an endometritis, a retroflexion, a prolapsus, or a sepsis that may be invited by a patulous os or by open vessels due to a lacerated cervix. Inertia and subinvolution are very generally due to conditions preceding normal pregnancy or that follow closely upon labor, and it is the object of this paper to demonstrate that for the most part they are preventable by treatment to be instituted during the puerperal period.

This is not the place to dwell upon the management of normal labor. It would be a very happy circumstance indeed were it possible for the surgeon to presume that a puerperal case should be attended under the same conditions as simple fever may be, but it has been demonstrated over and over that however beautiful the argument that labor is a normal process, and whatever strictures may be based upon what has often been styled meddlesome midwifery, the fact remains that the obstetrician who uses the same precautions as does the surgeon in the preparation of himself and the patient and the regulation of their environment will have the best results for both. But there are conditions dependent upon civilization that undermine the nerve forces of women that cannot be regulated nor always foreseen. There are conditions in the life of a woman, beginning with childhood, that seem to develop the nervous system to such an exquisite degree that the evolution of the reproductive apparatus is interfered with. The relation between the two is not considered to any such degree as the circumstances demand, but no one can observe the life of a woman through girlhood and maturity without being impressed with this fact. It is, of course,

in every case of faulty involution necessary to be sure that there is no retained placenta. It must be borne in mind that there may be present a fibroid, a lacerated cervix, valvular disease of the heart, and those rarer complications due to passive hyperemia dependent upon disease of other organs interfering with normal circulation.

The treatment of inertia is modified by the stages of labor. During the first stage there is no need, ordinarily, of active interference. Ergot is positively harmful to the mother in provoking tetanic spasms of pain, and to the fetus in disturbing the placental circulation. Rest by sleep is indicated more than anything else. Sometimes fifteen grains of chloral, repeated every quarter-hour, or one dose of thirty grains by rectum, may be particularly indicated. At times when worrisome and feeble pain is conspicuous, an eighth of morphia, hypodermatically, may be sufficient to give the patient a few hours to recuperate, when the labor will go on again in a normal course.

During the second stage, when inertia is present, if the cervix is dilated or dilatable, instrumental delivery may be the very best course of procedure. If, for any reason, that may not be advisable, the value of the slowly interrupted faradic current should not be overlooked. There need be no introduction of the electrode in the parturient canal; one pole, it matters not which, applied over the abdomen, the other at the lumbar spine, is equally effective. At this stage, if the primary current of quantity is distressful, the secondary one of tension may be substituted. The contraction will not then be marked by severe pain, but, on the other hand, the effect will be rather soothing. So far as my own observation goes, the pain complained of is due to the use of the current of quantity, which generally distinguishes the little portable apparatus provided in the obstetrical outfit. Strychnine, given in doses of a twentieth or fortieth, repeated at intervals of a couple of hours if necessary, will bring into action the influence of the spinal motor centres. At this stage of labor strychnine will not only aid in increasing the strength of the uterine contractions, but it serves as an invaluable prophylactic against postpartum hemorrhage. Ergot at this stage is very rarely to be used, but if used should be guarded in its dosage so that tetanic contracture is avoided. I believe I have oftentimes happily changed the course of labor by the administration of strychnine as noted above, and five minims of the fluid extract of ergot at intervals of fifteen to thirty minutes, always withholding it when rhythmic contractions were secured.

It is particularly when the third stage of labor is reached that the most serious consequences of inertia are manifest; and anyone who has observed a single case of postpartum hemorrhage, or perhaps has seen the life current ebb rapidly away before any means at hand could staunch the flow of blood, will understand how important is preventive treatment. I believe at this time the value of the faradic electric current should be emphasized. It is my own rule to always have at hand a compact portable faradic apparatus with the coil of coarse wire, and apply the poles to the lumbar and abdominal region the moment danger threatens. In my experience I have never failed to secure a vigorous response to a current applied in this way. Theoretically, the bipolar intrauterine application is more active, but I have thus far never had occasion to apply it.

The objection to introducing the pole into the cavity of the uterus is not of much weight, because, as a result of the manipulation of the patient, the application of ice, the introduction of vinegar, and the various interferences for the checking of hemorrhage, it will be necessary to flush out the cavity with an antiseptic solution, and the simple introduction of the properly constructed bipolar electrode need not carry with it any great objection. The telescopic electrode should have no place in an aseptic equipment. It is uncleanly and in its cavernous tube lurks danger. The extreme value of the electrical current at such times has in my own neighborhood been so often recognized that scores and scores of times have I been sent for by my confrères, with the request to bring a battery to aid in controlling a case of uterine relaxation. By no means let it be understood that reliance is placed upon electricity at this time, but were I deprived of it I would feel that I was seriously crippled in respect to my armament. While the battery is being prepared no time should be lost, but the placenta should be expressed and contraction secured by massage. At this time ergot has its precious place. It should never be given by the stomach. Again and again have I seen, after the situation was under control, the stomach eject the whiskey and ergot and whatever else may have been administered. The hypodermatic syringe should be at hand, precisely as we have our restoratives ready for instant use during the administration of an anesthetic. A valuable article in the obstetric outfit is the little hermetically sealed capsule of ergot for hypodermatic use. I wish at this point to emphasize the necessity of being sure that the uterine cavity is emptied not only of secundines, but also of coagula. If the hand has been introduced, this

makes necessary a final cleaning up with a 1:4000 sublimate douche. I believe that ordinarily the saline or plain water douche is not necessary following the use of the sublimate; the firm contraction and normal oozing that ought to follow will effectually guard the patient from any mercurial poisoning.

The woman who has been safely carried through her labor and is relieved from danger by hemorrhage, is to be guarded with extreme care and ought to be kept under the frequent observation of the doctor during the following weeks of the puerperium. I have had women walk into my office many weeks after having aborted, complaining of hemorrhage and pelvic distress, who upon sounding have been marked by a uterine depth of four to five inches. One time, four months after a labor at full term, a woman appeared with the uterus measuring seven inches. During the period of convalescence, if there has been any difficulty about determining the presence of a cervical laceration, there should be little time lost in settling that point. I have to again emphasize the value of the electrical current during this period. It has been my practice to show the nurse how to use the battery with the external electrodes and make daily application of fifteen minutes' duration, using the slowest interrupted current. I have always used the faradic battery. I have no doubt that the interrupted galvanic current would be equally efficient—theoretically it might be given the preference, or the two might be combined; but my results have been very satisfactory thus far from the use of faradism alone. It is to be noted that the cases I am discussing are marked by faulty physiologic action, but, strictly speaking, are not pathologic. There is no hyperplasia. There is passive and less often active hyperemia. There is stasis. The cases distinguished by chronicity promptly respond to the influence of the bipolar intrauterine faradic current. A uterus with a depth of five inches, four weeks after abortion, after four applications during two weeks, I have observed shrink down to a depth of three inches, hemorrhage meanwhile being arrested and the woman continuing her work. "The effects of the faradic current are mechanical and may be accurately regulated; while analogous to ergot, they are more prompt and energetic. The current is one of constant breaking and closing. It produces a sort of interstitial massage, heightening the activity of the circulation, accelerating absorptive processes and influencing favorably the nutrition of the parts. By its direct excitation of the smooth muscular fibers of the uterus we are enabled to combat the stasis of the circulation, which is the begin-

ning of uterine inflammation."[1] At this time, particularly after the first week, the hot-water vaginal douche, as outlined by Emmet, may be used with great advantage. The points always insisted upon while using it are the elevation of the hips, by which to secure the effects of gravity upon the circulation; the water at a temperature of from 110° F. to 115° F.; the continued flow for not less than fifteen minutes, used at least daily, sometimes morning and night; and the continued horizontal position for at least an hour afterward. Very much the same results may be obtained by the introduction of boroglyceride tampons, introduced daily or every other day, while the patient is in the Sims posture. Where a well-trained nurse is in attendance this may be left to her. The shrinking of the uterus that follows upon this treatment is oftentimes wonderful. I find that circumstances lead me at one time to prefer the hot water and at another time the vaginal tampon. Much depends upon our assistants; where the nurse is not very skilful and your own time is limited, you may be safer in using the douche than the tampon.

The use of ergot at this time is of decided value. The trouble to be overcome is lack of contraction in the non-striated muscular fibers of the uterus. The most pronounced action of ergot is just in this particular direction, so that it would seem to be the ideal drug. Its combination with hydrastis and sometimes with Indian hemp may be indicated. Nux vomica or, preferably, strychnine in cases marked by faulty innervation is of special value. In anemic cases iron is a strong ally; cinchona or quinine are good adjuvants. Fordyce Barker's preparation of nux vomica, iron and ergot is not a happy combination pharmaceutically. Few women will be willing to repeat the dose, and fewer stomachs will be able to take care of it. The treatment outlined by Tait has proved a very satisfactory one, and, I believe, is based upon sound considerations. I refer to the administration of ergot with citrate of potassium. The combination of potassium with the vegetable acids, it should be remembered, effects its elimination by the kidneys. Of all the potash salts I believe the citrate is the most acceptable to the stomach. It should be given in doses of from one-half to one drachm three or four times a day, preferably administered an hour before meal-time with a half-pint of water. There is a special indication for its use in sthenic cases that present a scanty flow of urine and very acid reaction, or with subjects manifestly of a rheumatic diathesis. Contraction of the muscu-

[1] Vide Hare's System of Therapeutics, section by Rockwell, or A. Lapthorn Smith's translation of Apostoli on "Metritis."

lature of the uterus and the elimination of retrograde products are the indications happily met by Tait's treatment. The influence of constipation must not be forgotten. Saline cathartics are particularly indicated in effecting depletion of the pelvis through the veins, and an occasional cholagogue may be necessary. There has been some doubt expressed as to the influence of lactation upon involution. Early lessons are impressive, and I may be biased by the practice observed twenty years ago when the old-time doctors insisted upon the frequent nursing of the infant to effect uterine contraction, notwithstanding the strenuous cries of the mother, whose uterine pains were at once invoked; yet I believe, whatever may be the theoretical consideration of the subject, the clinical evidence is all in favor of the influence of lactation in effecting involution, and that every mother should be encouraged to nurse her baby, in the hope that she will have a better getting-up in consequence. The unfortunate thing is, however, that here, as in the preceding stages of the puerperal period, the influence of asthenia again asserts itself, and lactation is too often missing.

I will conclude, as I began, with the statement that the puerperal period is the precious time for the prevention of subinvolution; that this fact should rivet the attention of the obstetrician upon the uterus and its functions of muscular contractions at all stages of labor and during convalescence. If this were more often considered and applied in practice the sequelæ of metritis, endometritis, retroflexion and prolapsus would be far less frequent.

INTRAPELVIC ADHESIONS.

By WALTER B. CHASE, M.D.,
NEW YORK CITY.

In a standard text-book on gynecology of about one thousand pages, less than one-third of a page is devoted to the distinctive topic of intrapelvic adhesions, and that *en passant;* another gives it no separate mention. It is surprising, on examination, to find how little notice it has received by authors and casual writers on gynecology and abdominal surgery—topics which in particular interest the members of this Association. While pelvic adhesions are frequently the accidents or accompaniments of varying pathologic conditions, it would seem, in matters of diagnosis and rational treatment, that their presence and influence are entitled to more careful and discriminating study and differentiation.

Any surgeon of considerable experience will, in a review of his cases, recall not a few in which adhesions were the principal factors of trouble, which were only revealed on opening the pelvic cavity; while in others, equally numerous, the adhesions were concomitants of pathologic states equally unexpected. These cases embrace wide extremes of symptoms, sometimes apparently out of all proportion to the gravity of the condition really existing. The failures to make correct diagnoses may arise from a variety of causes. Doubtless in the cases as they present themselves accurate diagnosis is, other things being equal, easier in females than in males, for the reason that bimanual palpation is more satisfactory and better reveals the true condition in the former than in the latter. Then, too, the thickness of the abdominal walls and the amount of adipose tissue, either intrapelvic or extrapelvic, render difficult the revelation of the underlying structures, their exact location, and accompanying deviation from a normal state.

So great is this embarrassment in the obese that diagnosis from physical signs is abridged or altogether abrogated. As an illustration of this difficulty I can cite two cases recently of omental

lipoma being mistaken for ovarian cyst in fleshy women by careful diagnosticians, and the abdomen in each instance was opened before the error became apparent. Another hinderance to an adequate and discriminating study of the pelvic cavity is muscular rigidity. While the recti muscles are the principal offenders, other muscular structures participate in the spasmodic action, giving the abdominal walls a tense, drum-like feeling. This rigidity is often sufficient to obscure conditions which otherwise would be easily determined. This rigidity may be voluntary or involuntary, subject to the control of the will wholly or partially, or due to reflex influences for which the patient is responsible. If voluntary, it may be overcome by gentle or persistently increased pressure, at the same time attracting the notice of the patient to some other consideration.

Muscular rigidity of the abdominal walls, localized it may be, has a special significance in appendicitis, ovaritis, and acute localized or general inflammations affecting the pelvic viscera. All these should be taken into account as preliminary, on which absolute or approximated diagnosis may be arrived at, on which to formulate a plan for correct treatment. How many failures and disappointments would have been avoided, and how much less scientific medicine would have suffered in the house of its friends, had such methods been uniformly pursued!

But, notwithstanding all the failures and mistakes of the past, progress is being made. It nevertheless remains true that he who fails from neglect is without excuse, and thereby hugs to his soul the instrument of his own punishment. In passing it might be profitable to consider somewhat more in detail the obstacles to the diagnosis of intrapelvic adhesions and the means of their avoidance. The character of the adhesions likely to be found must be included in the study of every individual case. Adhesions will vary according to the variety of the inflammation producing them and their plasticity, and this in turn will be dependent on the dyscrasia of the patient, while the proximity or remoteness of the period of their occurrence is a factor to be included in the analysis of each case.

Recent adhesions lack the strength and resistance of older ones, and those formed in the presence of septic conditions and in the exudations about tubercular and other degenerative deposits and malignant growths have a low order of vitality. Allusion has been made to the necessity of muscular relaxation as needful to

proper superficial and deep palpation and percussion in arriving at accurate conclusions, and, if needful, anesthesia should be resorted to for such purposes. The mobility of the pelvic contents perhaps furnishes more decisive information than any other single symptom or condition. Advantage will be found, in making the most of it, to vary the position of the patient from side to side from a horizontal to the upright posture, and in changing it again to the Trendelenburg, and from that to the knee-chest position. By such procedure, combining with it, it may be, percussion and palpation, and taking into account the presence or absence of pain, valuable information is elicited. In this procedure too forcible handling of the parts must be avoided. Doubtless rash efforts in this direction have ruptured many a pus sac, with its fatal consequences, while it remains equally true that careless or superficial examinations have failed to differentiate easily determined conditions. A careful and logical study of the antecedent history of the patient will afford valuable information. Has the patient suffered from previous peritonitis, salpingitis, metritis, or pelvic cellulitis, and, if so, how much has this contributed to the present status of the case? What sequelæ are due to pregnancy, sepsis, tuberculosis, syphilis, etc? Has traumatism been a factor in the pathological condition present, and, if so, how is it responsible for impaired health or threatened invalidism? If, when all these and other details have been systematically studied and deductions formulated, including with the physical signs all the light a thorough analysis of the rationa symptoms afford, doubt and uncertainty remain, resort to abdominal section may be needful to establish a certitude in diagnosis. While a careful application of these principles for purposes of general diagnosis always obtains, their necessity in cases of suspected adhesions is specially demanded. It is only by such a course of observation and deduction that proper knowledge of intrapelvic adhesions can be diagnosticated. Should abdominal section be decided upon to complete the diagnosis in case of expected adhesion, the operator may, with the consent of the patient or friends of the patient, be prepared to do whatever is found needful. It is almost superfluous to mention that such procedure assumes that needful preliminary steps have been taken to put the patient in the best possible condition for operative interference.

The study of the nature and influence of intrapelvic adhesions would be an interesting and fruitful topic for this entire paper, but cannot be dwelt on to any considerable length. The deleterious

influence arising from their presence is of a mixed character, being partly functional, partly mechanical. The mechanical disability, so far as the intestines are concerned, arises from narrowing of the lumen of the gut, angulation and interference with normal peristalsis, all of which tends to the development of constipation and consequent fecal accumulation and fecal impaction. The discomfort and disability arising from constipation is closely allied with ailments far-reaching in their effects and consequences. Constipation is doubtless a fruitful cause of fecal poisoning by absorption of ptomaïnes, which play so important a rôle in the development of anemic and lithemic states of the system and some undefined disturbances of intestinal digestion and nutrition, and also in the production of varicose conditions of the hemorrhoidal veins, chronic proctitis and rectal ulceration, together with the venous congestion of the whole pelvic contents and return pelvic circulation. Adhesions of the bladder to the uterus, ovary, broad ligament, or intestines give rise to an aggravated form of cystitis and cystic irritability difficult of cure except by removing the cause. The vasomotor disturbances and reflex irritation accompanying these conditions present another perplexing feature of the lesion which clamors for relief. Among the simpler forms of intrapelvic adhesions are the presence of bands which, by their progressive contraction, obstruct the bowels. The symptoms of this will vary according as it is an acute or chronic condition, and are usually too simple to give difficulty in correct diagnosis. Unfortunately it is this class of cases in which the plain indications for prompt interference are disregarded with appalling consequences. Repeatedly in the past few months have cases come under my observation, both in hospital and private practice, where the golden opportunity for conservative interference had passed.

On opening the pelvic cavity the first adhesion encountered is that between the abdominal peritoneum and the omentum, making difficult, in some cases, certainty when the peritoneal cover has been passed. The adhesions may be between omentum and the intestinal peritoneum, between intestine and intestine, intestine and ovary, tube, appendix, etc., sometimes to a degree which amalgamates all the intrapelvic structures, displacing altogether the normal relations of these organs. You are all familiar with the wide variations these adhesions present. In one case an ovary is constricted by a single band of lymph, which has resulted in persistent and uncontrollable neuralgia. In another the ovary is

attached to intestine, uterus, or appendix ; the uterus is held in abnormal backward, forward, or lateral position. Again, these several organs are adherent to a neoplasm, or pus-tube, or abscess sacs, presenting in variety of detail a multiplicity too great for present enumeration. What shall be done with them? On the judicious answer to this question will hinge the whole theory and practice of rational procedure. Sometimes the surgeon will respect them and let them alone. Again, their blight and vicious influence will be the signal for his attack. Where their presence is general it is obvious their obliteration is outside the limit of conservative surgery ; yet in cases not a few the removing of tension at some point will afford measurable relief. The adhesions which will tax the skill and dexterity of the operator are those covering in old abscesses, holding in position intraligamentous growths, and the new adhesion between the living placenta and intestine following the rupture of ectopic gestation, and those encountered in intestinal resection.

In view of the disability arising from these conditions, it is well to raise the inquiry, What can be done to prevent their development? A comprehensive answer would be to arrest the septic and inflammatory processes on which their presence depends. It has long been recognized that two drugs possessed greater or less influence on the development of new connective structure, potassium iodide and mercurials, to which may be added one of the newer preparations, gold and sodium chloride. The judicious and timely use of these remedies may arrest the processes which make for injury.

The success of the Brandt method of pelvic massage has, it seems, claims on our recognition, but I am unable to speak from personal experience. Usually when these cases of extensive adhesions reach the observer the mischief has been done and the appeal is made for surgical interference. It is, however, well to remember in some cases that time will bring a degree of relief, while in others it increases the suffering and disability. The danger of dealing, *per se*, with those adhesions is twofold : First, the liability to hemorrhage, and, second, the liability of new adhesions forming. When practicable, if the adhesive bands are of sufficient size, they should be severed as near their origin or insertion as possible, the blood-vessels tied, and the cut surfaces turned in on themselves and stitched over with the finest catgut. This reduces the risk of new adhesions to the minimum. The principal embarrassment in actual

operation arises from bleeding and oozing from the hyperemic condition of the parts. Particularly is this true in chronic and subacute inflammatory states. Under certain conditions the tearing across so lacerates the vessels as to prevent bleeding. In some cases the oozing can be controlled by combined pressure and hot-water sponging.

When torsion or ligation of the vessels is impossible and other devices have failed, it occasionally becomes necessary to apply more permanent pressure by gauze packing, which must be allowed to remain until the vessels are occluded. This, of necessity, prevents closing the peritoneal cavity both in abdominal and vaginal section. The liability to new adhesions succeeding old ones seems to some degree to be diminished by sponging the location of the old adhesions, after their separation, with hot salt solution or allowing a portion of it to remain in the peritoneal cavity, when not contraindicated.

Finally, we have encouragement for the future in the application of electro-hemostasis, so ably discussed by Dr. Skene in his work entitled *Electro-hemostasis in Operative Surgery*.

POSTPARTUM REPAIR OF LACERATIONS OF THE CERVIX UTERI.

By CLINTON CUSHING, M.D.,
WASHINGTON, D. C.

Probably few physicians have a clear idea of the injury to the cervix uteri in many cases of what are considered normal labors. My reasons for this belief are based upon my personal experience in past years, before I realized the frequency of cervical injuries, and it was only after careful investigation of a number of cases with a Sims speculum that I began to appreciate how very serious their injuries may be and still cause, for the time being, so few symptoms. The text-books and writers on gynecology merely mention the fact that lacerations of the cervix uteri frequently occur in varying degrees at the time of labor, but the subject is dismissed in a few lines and no mention is made of the necessity of a careful ocular examination of the parts. Nearly all agree that lacerations that are allowed to heal by granulation result in the formation of scar tissue, causing hypertrophy, hyperesthesia, disturbed function, and troublesome reflex symptoms.

The common opinion is, also, that an old, unhealed laceration of the cervix, covered with granulation and bathed in a mucopurulent secretion, is a common exciting cause of cancer of the part. It is a fact that cancer of the cervix is extremely rare in women who have never been pregnant, for I have never seen such a case. Emmet has also placed upon record that at the time he published the first edition of his work on gynecology, all the cases of cancer of the cervix that had entered his private hospital, or had come under his observation suffering from an epithelioma of the uterus, had in every instance been pregnant.

In late years I had nearly given up the practice of obstetrics, for my time and energies have been occupied in surgical matters; but the few women whom I have attended in accouchement I have carefully examined with a Sims speculum, in order to determine the

extent of the injury, if any, and I have been amazed at the extent of the lacerations. In several instances the rent extended laterally into one or both of the broad ligaments sufficiently far that three fingers could be easily laid in the gap, the torn space being filled with blood clots, fragments of membranes, and débris; and before the exact nature of the injury could be appreciated all the mass of refuse had to be sponged and wiped away. It is a fallacy to think that a correct diagnosis of a laceration of the cervix or perineum can be made by a digital examination.

In many cases the parts have been so much overstretched and bruised and swollen that the information given by the sense of touch is very indefinite and unsatisfactory, and it is by an ocular examination only that an intelligent opinion can be gained. I doubt not that if any of the members of this Association who attend women in confinement, will take the trouble to examine properly the next half-dozen cases under their care they will be surprised at the number and extent of the injuries found. The examination should be made within twenty-four hours following delivery, and, unless there be great exhaustion or some other good reason, it should be made within an hour or two after the birth is completed. The examination should be made in such a manner as to cause very little annoyance or discomfort to the patient; and if good reasons are given the patient, and the statement made that it is important that she be left in as perfect a state as possible for her future health and usefulness, no objections will be raised. The danger from an examination need not be seriously considered, if it is taken for granted that the accoucheur, the nurse, and the appliances are positively clean—that is, clean from a surgical stand-point.

If it can be arranged, it is much better to lift the patient on to a table, but if this is not feasible the examination can be made upon the bedside; but, in any event, a good light is indispensable—either an electric or sunlight, or, in an emergency, a kerosene lamp. All instruments should be boiled before using, also the water used in washing the hands or the patient. The patient should be placed in the left lateral position, and, after a digital examination, a Sims speculum should be introduced, and, with a forceps and absorbent cotton, blood and secretions removed from the vagina; and now, by seizing the anterior lip of the cervix with a bullet forceps, the cervix is under control and can be drawn down, if needed, so that all parts can be easily seen and any rents wiped dry and inspected. If the injuries are severe, and especially if the tear extends beyond the

cervix into the connective tissue, it should be repaired at once. Any ragged or irregular surfaces should be trimmed away with scissors and the parts united with interrupted sutures of formalin catgut. The woman should now be turned upon her back, and any injuries of the perineum should be repaired with silkworm-gut and perforated shot, the ends of the sutures being cut off on a level with the shot, each shot having attached to it a tag of strong black silk thread an inch in length to facilitate the removal of the sutures. After carefully douching out the vagina and afterward drying it as perfectly as possible, the vagina is filled, by means of a teaspoon, with a preparation that has been of great comfort to my patients and of immense satisfaction to myself. To each ounce of freshly and properly made oxide of zinc ointment are added a grain of morphine and a grain of cocaine. The vagina is filled with this mixture, and a large teaspoonful is also introduced into the rectum, which usually suffers more or less in the process of childbirth. The woman is now put to bed, and, with the exception of douching off the vulva after urination, no treatment is required. The presence of the ointment in the vagina, slightly astringent and soothing, favors the healing by first intention and no vaginal injection is permitted and no manipulations required. The gradual oozing of the ointment keeps the repaired perineum protected and prevents any urine from entering the vagina or contaminating the perineal wound. At the end of nine days a vaginal douche of warm carbolized water is given and the perineal sutures removed, the black silk tags in the shot materially facilitating the location and removal of the sutures without undue separation of the newly united parts. No attention is paid to the cervical sutures. The results have been all that could be desired: the parts have healed kindly, and the women have been restored, as far as the cervix and perineum are concerned, practically to the condition they were in before impregnation.

What has been said might leave the impression that the repair of the cervix postpartum is an undertaking of some magnitude. Not so; it is simplicity itself. In anticipation of the apprehended injuries, the patient, at the orders of the medical attendant, provides beforehand the necessary remedies and appliances, so that in case of need no time is lost; and this preparation also gives the woman to understand that nothing is left undone to provide for her safety and security, increases her confidence in her physician, and renders her more willing to submit to whatever may be thought best for her. Anesthetics are not ordinarily needed for the repair of the cervix.

Happily for woman, during the child-bearing period of her life the cervix is one of the most insensitive parts of the body, and now the sensations of the part are still more obtunded by the pressure; the stretching and the tearing render the introduction of the sutures, if not painless, at least tolerable. Not so the perineum, for it is one of the most sensitive parts of the body, and the last to lose sensibility under ether, but its sensibility is greatly lessened by the task imposed upon it by child-birth. The judgment of the physician will decide upon the necessity of anesthetics, and must depend upon all the facts in the case. If we are to judge of the danger to life and health of the comparative importance of lacerations of the cervix and perineum, we must certainly decide that the cervical injuries are by all means of the most consequence. Cancer of the perineum is rarely heard of, cancer of the cervix is of daily occurrence.

I am the more willing to present this subject for consideration and discussion, firstly, because I believe it to be a matter of importance, and, secondly, I am not aware that it has been heretofore presented.

THE PRESENT POSITION OF GALLSTONE SURGERY.

With a Report of Cases.

By WILLIAM WOTKYNS SEYMOUR, M.D.,
TROY.

My 27 cases of operation for gallstones are extremely small in number when compared with the 468 cases of Kehr, but they have been drawn, with two exceptions, from practice in a city of 70,000 inhabitants, and represent but a small part of the cases which I have seen and in which I have advised operation for the relief of gallstone attacks. As I remarked in a paper before the Association several years ago, we do not know the actual mortality of gallstone disease, so many dying under other diagnoses and many dying from other conditions not directly associated with gallstones in the minds of the attendants.

This latter possibility I cannot better illustrate than by the history of three cases coming under my observation. Several years ago I was stopped on the street by a professional friend and asked to go to the relief of a case of intestinal obstruction. I immediately procured needed instruments and dressings, and found a patient with a strangulated right femoral hernia, until then unrecognized. Her history was that six months before, after a sudden attack of very severe epigastric pain and vomiting, a lump appeared in the right groin, but went back when the attacks of pain and vomiting ceased, as they did in a few days. Six weeks before my visit the epigastric pain and vomiting had recurred, and the lump had reappeared and had not gone back on the cessation of pain and vomiting a few days later. Three days before my visit the pain and vomiting had again come on, and had never ceased, the vomiting being fecal at the time of my visit. The eyes were slightly icteric, but the skin merely muddy; stools had not recently been clay-colored. My diagnosis was strangulated femoral hernia due to the retching attendant upon gallstone colic. Immediate operation released a knuckle of small

intestine, the color of which so improved under hot douching that I believed it to be safe to drop it back into the belly. For thirty-six hours all seemed to go well, when the temperature began to rise, a soft systolic murmur, not formerly heard, was heard at the apex, and delirium set in. There were no symptoms of peritonitis. The murmur increased in audibility, and the patient died the fourth day. My diagnosis before death was septic endocarditis secondary to femoral hernia following gallstone colic. The autopsy made by my friend Dr. H. C. Gordinier showed no peritonitis, a pin-hole perforation in the eburnated line of the strangulation, belly clean; the endocardium was crimson, and there were shallow ulcerations on mitral valves. In the distended gall-bladder was a solitary stone the size of a pigeon's egg.

CASE II.—Man weighing 230 pounds, and fifty-four years of age, of tremendous frame, who had had, under Dr. Donald Buchanan's care, what he had diagnosticated as gallstone colic lasting several days and accompanied by severe vomiting and retching. Suddenly a mass appeared in the right groin and scrotum. This could not be reduced, and both pain and vomiting increased; the pain now being in the groin, constipation absolute. Dr. Buchanan, with my assistance, operated and relieved a strangulated omental hernia, and in so doing was obliged to remove a large mass of thrombosed omentum. Vomiting and pain subsided, and all seemed to be going nicely, when, about two weeks after the operation, the patient began to have high fever and pain in the region of gall-bladder. Skin intensely jaundiced, urine porter-colored, and stools clayey. A dulness could now be traced from two inches to the left of the median line across to the right a little beyond the right mammary line, and vertically from the liver border to nearly the crest of the ilium. Palpation showed exquisite tenderness and a sense of deep fluctuation. He entered the Samaritan Hospital, where I opened the mass over its most prominent part, about two inches to the right of the navel and two and a half inches above. At least two quarts of pus and blood clot with necrotic shreds were evacuated. The nature of the mass I could not determine. It was above the transverse colon and below the liver border. The erosion of the wounded edges from the discharge led me to suspect a pancreatic cyst, but this I could not prove. The patient made a rapid recovery.

CASE III.—German saloon-keeper. Repeated severe attacks of gallstone colic of great severity. Complete obstruction of common duct. Operation advised and declined. Jaundice and putty-colored

stools continued several months, then the liver began to enlarge until it reached nearly to the anterior superior spinous process of ilium. This was followed by contraction, dropsy of belly and legs. Diagnosis, biliary cirrhosis. Death. No autopsy could be obtained.

Now, although the second case recovered, it was clearly connected with the original gallstone attack, and the situation of the fluid collection makes the connection still more probable, whether due to a perforation of a gall duct, a gall-bladder abscess, or a pancreatic hemorrhage or cyst.

In the third case, despite the occupation of the patient, he was not an alcoholic, and the sequence of complaints was unbroken from the gallstone colic until death from cirrhosis of liver.

I have gone at some length with these cases, because, had they all died, gallstones would have been as much the primary cause of death as the last pathological diagnosis.

All but 5 of my 27 cases were in women. Of the women, the youngest was twenty-two and eldest sixty-nine; of the men, the youngest twenty-three and eldest fifty-nine. In 2 women the stones were solitary and in 2 of the men; in women, confined to the gall-bladder and by their ball-valve action exciting paroxysms. In 3 women, in addition to the removal of stones in the gall-bladder, I crushed stones in the common duct. These all recovered without trouble. In 2 men and 1 woman I opened the common duct and removed stones from the common duct by incision of the duct without suturing the duct, and all recovered. The histories are briefly as follows:

CASE I.—Woman, aged fifty-nine years; married; for years had suffered from so-called neuralgia of stomach. At time of operation intensely jaundiced and mere skin and bone. Dense adhesion between transverse colon, liver, and stomach. After separating them no gall-bladder could be found, but deep in belly, between liver and transverse colon and above the foramen of Winslow, a mass could be felt which apparently contained stones. This was incised between artery clamps and some one hundred pea-sized stones removed; a finger introduced into the opening passed upward into the hepatic duct and downward toward the duodenum, while to the right was an opening admitting the finger into a gall-bladder contracted to the size of a hickory-nut. Drainage-tube was passed into hepatic duct and a glass tube threaded over it and safeguarded by tampon of sterilized gauze. Normal recovery. Fistula closed in sixteen weeks.

CASE II.—Machinist, thirty-seven years old. Patient had "belly-ache" frequently for years, attacks lasting one to three hours, occurring at all times. Never jaundiced until present attack, which began two weeks before my visit. Pain occurred daily, and at time of my first visit the patient was decidedly yellow and stools putty-colored. Emaciation and loss of strength, owing to the continued pain and loss of appetite, were so great that the patient assented to operation. After breaking up very firm adhesions between transverse colon and liver border, a semi-fluctuating mass was found in the situation of the common duct. Incision of this gave exit to several ounces of dark-green bile, and the liver, which till then was a *dark olive-green, became reddish.* A single round stone half an inch in diameter was found low down in the common duct. The common and hepatic ducts were dilated to the size of the thumb, and from them the little finger could be passed into a very much contracted gall-bladder. Rubber drainage in hepatic duct, glass drainage over it to the wound in common duct, all safeguarded by aseptic gauze tampon. The rubber drainage was accidentally displaced the second day. The glass drainage was removed the fifth day and the gauze the seventh and eighth days. The entire wound was tightly closed the seventeenth day. Stools did not become colored until after several days, and it was a fortnight before the jaundice had greatly diminished. Patient has since been in perfect health.

CASE III.—Physician, aged fifty-nine years; always in robust health until within eighteen months, when he began to have pain in the stomach, which he ascribed to dyspepsia. In June, 1897, he first consulted me, when I diagnosed gallstones and advised operation, but he would have none of it—in fact, did not believe in it. In December, 1897, he again consulted me. Pain had occurred very frequently of late—sometimes two or three attacks in a day—with marked loss of appetite, intense jaundice, and putty stools. Now the patient believed he had cancer of stomach or of liver. My diagnosis was complete obstruction of the common duct by stones. Three days later I operated at the doctor's home in Vermont. Owing to the stoutness of the patient the operation was a very difficult one. Adhesions were numerous, but easily separated; the gall-bladder contained three ounces of dark bile and tea-ground-like sediment, but no stones. The common duct was found completely jammed with stones. Carefully incising it, ten stones were removed. Duct wound not sutured. Gall-bladder stitched to the parietes and drained with rubber tube. Rubber tube in hepatic duct, glass tube

over it to common duct. Tampon to safeguard it all. Temperature and pulse in three weeks never above normal. The only trouble was the patient bullying his doctors and nurses for water and morphine until I read the riot act and prohibited morphine, which had been given him. An abscess formed about a broken silver suture. The fistula did not close until the twenty-sixth week, which I ascribed to an overlooked stone subsequently passed. The patient is now free from pain and in perfect health, carrying on a country practice.

My experience has been that in the vast majority of cases simple cholecystotomy with drainage of the gall-bladder after the method of Tait is the operation of choice, inasmuch as it removes the stones from the most common situation, the gall-bladder, and by the drainage carries off the infected contents of the bile canals. In the Heidelberg clinic the contents of the gall-bladder were shown almost without exception to be teeming with bacteria when first opened, yet after three weeks' drainage every one was sterile. The occasional occurrence of typhoid cholecystitis is another reason for prolonged drainage. This last spring I saw with my friend Dr. H. C. Gordinier a German woman of fifty years of age who had numerous severe attacks of gallstone colic. After the last attacks, lasting samewhat over two weeks, with daily paroxysms, the temperature rose to 104°, and the patient was intensely jaundiced and the enlarged gall-bladder exquisitely tender. My opinion was that we either had to do with a suppurating cholangeitis or typhoid infection. Widal's test showed typhoid reaction, and the patient had a regular course of typhoid fever. Operation declined after recovery from typhoid. The general typhoid infection of course contraindicated an operation. But in view of the experience of Maurice Richardson and others with cholecystitis, any subsequent operation will demand an efficient drainage of the gall-bladder and ducts if one would remove the typhoid germs, which have been found as late as eighteen years after an attack. In view of the success now attending the opening of the common duct, our views regarding excision of the gall-bladder must be modified. Formerly most operators believed it to be indicated only in malignant, gangrenous, or ulcerative conditions. But inasmuch as stones are most frequently found in the gall-bladder alone, there may be anatomical conditions, such as contracted gall-bladder and dangerous traction upon the stomach and duodenum, if the gall-bladder is sutured to the parietes, which will make the excision an operation of choice over the simple Tait

operation. However, before undertaking an excision we must be confident that the common duct is not occluded, or we invite disaster by the operation. If there is any doubt, one had better do a cystotomy and drain with tube and gauze rather than to incur a dangerous degree of traction or to run the risk of leaving occluded bile channels.

Stones in the cystic duct, unless they can be easily pushed back into the gall-bladder, are best removed by simple incision of the duct, either with or without its suturing.

The old operations of crushing *in situ*, as done by Tait and Kocher, or needling, as done by Thornton, no longer deserve acceptation, as they may leave in the ducts débris to become the nidus of formidable concretions. A clean incision and removal of unbroken stones is vastly the most surgical procedure, now that we can close the opening with suture or safeguard it by drainage and tampon. In view of the bacteriological results from the Heidelberg clinic, I, who personally have been led to draining the common and hepatic ducts instead of suturing them, am the more confirmed in the propriety of this procedure, instead of contenting myself with the suture of the duct after removal of the stones. In none of my cases have I seen McBurney's operation of opening the common duct through the duodenum indicated, but that it has a field in the case of stones near the intestinal opening of the duct is certain. A post-duodenal or transpancreatic incision is much more dangerous, the first by reason of the arteries surrounding the duct, and the second by the added danger of a pancreatic fistula. In the few cases in which I have tried the duodenal incision on the cadaver it did not appear in the medium belly very difficult. McBurney's cases, and, I believe, Kocher's, have all recovered. It is from the cases of stones in the common and hepatic ducts that we are to expect our greatest number of so-called "recurrences." I say so-called "recurrences," because I believe that in most instances we have to deal, instead of a true re-formation of gallstones, with a stone which has been overlooked, washed down from the hepatic ducts, or of which the nidus has been a fragment left by crushing a stone during a previous operation. Of 27 cases I know of but one in which there was a subsequent attack, and that was in a case in which, in opening the common duct, I crushed some of the stones completely plugging it. The fistula was slow in closing, and I am inclined to think I did not remove all the fragments. My twenty-sixth case and my second fatal case was one illustrating a class of cases which one will from

time to time meet in a large series. German woman, fifty-five years old, very feeble and frail; gallstone attacks for many years, diagnosticated as dyspepsia, neuralgia of the stomach, etc., *id omne genus*. Came under my observation a fortnight before operation with paroxysms of great severity occurring two or three times a day; complete anorexia. Nothing seemed to control the attacks, and vomiting occurred so often that I counselled operation as the only hope. The stomach was dilated, but no mass could be felt. At the operation the omentum and transverse colon were found intimately adherent to the liver, and on breaking these up the gall-bladder was found adherent to the duodenum and stomach by dense adhesions; these, partly by knife and partly by blunt dissection, I freed, and removed from the gall-bladder thirty-seven stones, several the size of almonds of the most irregular, flattened quadrilateral shape. I wished to do a gastro-enterostomy, but the patient's condition demanded a rapid completion of the operation, and I contented myself with attaching the gall-bladder to the parietes, with drainage, and closed the wound. Pain there was none to mention; the bowels moved and flatus passed, but the stomach did not take up its work.

Food would be retained for twenty-four hours, and then vomiting would come on. Without any considerable elevation of temperature, this condition lasted just a fortnight, when the patient died of the results of a dilated stomach. In just such cases as these Kehr has wisely and successfully combined his gallstone operations with gastro-enterostomy and pyloroplasty. On reading the report of his cases in the *Archiv. für klin. Chirurgie*, I was greatly impressed with the wisdom of this addition to our means of attack upon cases of long duration in which cholecystitis and local peritonitis, by adhesions and bands, had changed the normal physiological relations to one of the hardest problems presented to an operating surgeon. If these cases are to be prevented early operation is the only way to prevent them; but when they exist the patient must incur the risk of an additional operation. The extent and density of the adhesions in chronic cases is sometimes astounding, and Kehr's combination of gastro-enterostomy and pyloroplasty with the accepted forms of gallstone operations greatly extends the field and increases the success of operations in cases otherwise hopeless. A permanent fistula means in most cases either improper suturing of the gall-bladder to the integument and not to the peritoneum and muscular fascia, or a stone has been overlooked. In the cases in which the permanent fistula seems to be due to an insurmountable obstruction

in the common duct, we are warranted in joining the gall-bladder to the intestine. But in view of cases of infection of the biliary apparatus in some of these cases in the practice of French surgeons, and our present knowledge of the frequency of intestinal infection through the common duct, I, for one, believe that making a fistula between the gall-bladder and intestine will be very rarely indicated and never ought to be done to side-track gallstones in the common duct.

To sum up, we have:

1. In Tait's operation of simple cholecystotomy with drainage of the gall-bladder the really ideal operation for gallstones in most cases, removing, as it does, the stones, and draining infected bile canals, and leaving no sutures as a nidus for another crop of stones, as Kehr and Homans have experienced.

2. In incision of the common and cystic ducts is the safest and most surgical means of removing stones in them.

The question of sutures or drainage of the ducts must be decided by the future.

3. In view of the splendid results of incision of the ducts for obstructing stones, excision of the gall-bladder may find a wider field than heretofore.

4. McBurney has shown that incision of the duodenum and either dilatation or incision of the common duct through this incision is, in skilled hands, both efficient and safe for the removal of stones low down in the common duct.

5. In neglected cases with dense and many adhesions and dilated stomach, an additional gastro-enterostomy or pyloroplasty will save cases which would otherwise die.

6. The mortality of the simple cases is practically *nil*. Even in my 27 cases I have had but two deaths directly connected with the operation. In one the patient, treated for weeks for cancer of the stomach and intensely jaundiced, died the fifth day of cholemia; and in the second case the cause of death was a dilated stomach in which a gastro-enterostomy would have given a vastly better chance. A third case was convalescent from the operation when attacked by a fatal attack of grippe two weeks after the operation.

ACCIDENTAL VAGINAL HYSTERECTOMY DURING DELIVERY.

By JOHN M. DUFF, M.D.,
PITTSBURG.

I DESIRE to report a somewhat unique case of vaginal hysterectomy accidentally performed during labor. I incidentally reported the case to the meeting of the American Medical Association because of its uniqueness and of there being no similar case on record, so far as I am able to learn from investigation.

The case in question was a woman near confinement for the third time. She went to the home of her father, where she could be attended by the family physician. He was called in the evening about ten o'clock, and found on examination a breech presentation. Pains were very violent, but ineffective, and inasmuch as she was suffering very much he concluded to give an anesthetic, and in order to do so sent for a neighboring physician of good reputation, who had been in practice for some forty years. Physician No. 2 came. Physician No. 1 gave the anesthetic, and No. 2 proceeded to deliver the child. He delivered the body of the child, but in doing so in some way decapitated and allowed the head to remain in the uterus. He then applied forceps to the head and attempted to deliver it. Failing to do so, Physicians Nos. 3 and 4 were sent for. No. 3 came, performed a craniotomy and delivered the head. I do not know what No. 4 did. The head was delivered, and the same physician delivered the placenta. They all left the house about five o'clock in the morning, very tired, with the woman in apparently good condition. A few hours after the attending physician returned, and on examination found something protruding from the vagina. It was intestines, and he immediately telephoned for me. I could not respond to the call just then, but I went three hours later to his office, and found him in bed sleeping. He came

down, and I asked him what was the matter. He said he had a case of what he thought was rupture of the uterus. He related the history I have just given you. We proceeded to the house, and on entering the room I found the woman in bed in a much better condition than could be expected. She declared herself to be in good condition. She had a good pulse, temperature was normal, and she was not apparently suffering from shock. I made a careful examination and found considerable bowel protruding from the vagina and torn off from its mesenteric attachment. The surroundings were very bad, and I suggested that she be taken to the hospital at once. Without any further examination I pushed the bowel up and packed gauze into the vagina, and had her taken to the West Penn Hospital in Pittsburg, where I had invited in the meantime several Fellows of this Association and others to be present at the time I was ready to operate. She was taken to the hospital at three o'clock in the afternoon. On examination per vagina I could not find the os uteri; after a little further examination I could not even determine where the uterus was. My hand would go up into the abdominal cavity, but I could not find it. I then opened the abdomen, took out several feet of bowel that was gangrenous, put in a Murphy button, and made an anastomosis, and then proceeded to search for the uterus, but it had gone. It had been amputated at the vaginal attachment as nicely as if it had been cut off with a knife. I then flushed out the abdomen, as I thought it was necessary under the circumstances, the fluid passing out through the vagina as well as through the opening in the abdominal wall, and then I told the nurse to count the sponges. There was one sponge missing for a little while, and I began to hunt around in the abdomen and felt something under the spleen. I said to myself, here it is, having slipped up there, and I reached up and pulled out the placenta from under the spleen. I sewed up the abdomen, and put the patient in as good condition as possible. She rallied from the anesthetic, and, strange to say, regained consciousness and talked in a rational manner with the nurse and attendants, and lived until the next morning, when she suddenly expired.

The uniqueness of this case is my object in reporting it, and the Fellows will excuse me for not mentioning names, which, under the circumstances, would not be the proper thing to do.

OBSERVATIONS RESPECTING THE SYMPTOMS AND TREATMENT OF THE MENOPAUSE.

By AUGUSTUS P. CLARKE, M.D.,
CAMBRIDGE.

Menstruation in the normal woman appears to embrace the period from puberty to the age of forty-five to fifty years. The flow undoubtedly comes from the vascular tissue of the Fallopian tubes, uterus, and vagina. It lasts usually in each menstrual period from three to five days.

The functional activity of woman partakes more or less of the character of that of the females of other mammalia, and it is, therefore, along these lines of research and by observations made in this direction that many of the most abstruse and perplexing questions pertaining to the phenomena of menstruation will have to be studied for effecting their solution.

In carrying on the work of investigation of this nature, too much regard cannot be had to the influences of the impress that has been imparted to the tissues of the female sexual organs through the almost infinite number of ancestral types. No single or limited number of classes of such agency will suffice for determining the settlement of all the questions involved. The separation of the ova from the ovary of the oviparous animals was early observed. Separation and discharge of ova from the mammalia take place without the direct interposition of the male.

Menstruation has been noticed to appear without the occurrence of ovulation. In such cases observation shows that there has been some morbid condition or defect of the tunica albuginea of the ovary. This has sometimes been the result of inflammatory changes taking place in the ovary, causing such thickening or alteration of the fibrous layer below the epithelial covering as to prevent rupture and escape of the Graafian vesicles. Such cases are not infrequently met with, and they require the most careful consideration as to their management. The presence of that condition may be followed with

constitutional symptoms and with the discharge of blood from the vagina, uterus, or the Fallopian tubes; but, owing to the organic changes incident to the morbid process, the general symptoms are liable to be more protracted and the inflammatory changes of the ovary to become more deeply diffused and of greater severity. There is undoubtedly a close relationship between menstruation and the discharge of the ova.

In those cases in which thickening or alteration of the tunica albuginea has taken place, the continuance of the function, if it is not thereby wholly arrested, is often seriously interfered with. This may give rise to an irregular flow, as is sometimes seen in the younger women who suffer more or less from nervous disturbance or perverted sensation. Whether menstruation in every case is accompanied by the formation and discharge of ova or not, it must be admitted that these phenomena are dependent upon the results of an evolutional process that has, though variously modified, been transmitted from the earlier and lower forms of the mammalia.

There are numerous factors, that serve to hasten, change, or modify the state of the organism, attending the approach of the menopause. The fact that it can be artificially induced or hastened by the removal of the adnexa or by resort to other surgical expedients, shows that the seeming regular appearance of the menses in woman is but the exercise of a transitional function that has obtained in the long physical history of the race, and that the irregularity of the phenomenon is often not so much an indication of disease as it is that the organism has been exposed to new and unaccustomed factors.

The approach of the menopause is not infrequently attended with a variety of symptoms. Besides the occurrence of flushes of heat, the digestive derangements and nervous depression, there is sometimes developed a class of perversions or prodromes that may be said to belong to the pre-menopause state. This may consist of a marked exaltation of the faculties and an exuberance of the imagination. This phase of the nervous system may supervene before the appearance of that degree of stoutness of the individual which is characteristic of commencing menstrual cessation. In not a few cases I have observed this peculiar phenomenon, and I have come to look upon it as a sign of ominous import. Outbursts of insanity may be a sequel of this condition, so also may attempts to undertake unequal tasks, the contracting of uncongenial marriage, neglect of family, the formation of the habit for the stronger stimulants, and the substitu-

tion of personal inattention for thoughtfulness, for neatness of appearance, and for the exhibition of proper domestic concern.

Undoubtedly the withdrawing to a considerable extent of the blood from the sexual system causes a greater distribution of that element to the brain or central nervous system; this may lead to an undue strain, tension of the vascular tissue of those parts, and thus lay the foundation, in impressionable or hypersensitive subjects, of premature organic and degenerative changes.

In two cases, at least, I have observed some facts tending to show that Colles's law of immunity did not obtain. In both cases the fathers had had syphilis and the children were syphilitic, but the mothers had escaped until the approach of the menopause, when marked constitutional degeneration from that infection gradually became manifest. A resort, however, to specific appropriate treatment quickly brought the disease under control.

An important symptom that should not be overlooked is the recurrence of hemorrhage after the term of menopause has been reached. When attention has been called to this feature of a case, an examination should be made in order to ascertain the exact condition of the cervix uteri and to determine whether there is any thickening, induration, or preternatural fixation of the uterus; in those cases in which there has been extensive laceration of the cervix, either externally or within the uterine canal, evidence of seriously marked involvement should be carefully looked for.

Here it may be remarked that the advice cannot be too much repeated that all injuries or traumatic lesions about the cervix should be attended to as early as possible, lest these parts may, on account of long-continued irritation, become the seat of cancerous or malignant development. The view here stated is not a mere hypothesis, but has become an established fact from the record of numerous well-observed cases. In a series of forty-eight cases of cancer of the cervix coming under my care within a recent period, forty-four occurred in women who had sustained cervical lesions; the other four cases appeared in patients who had suffered from dysmenorrhea and from other continued uterine disorders. The above-mentioned cases could, in all probability, have largely been prevented by resort to surgical measures before the period of the menopause had been reached.

Epistaxis is another symptom I have especially observed. In those cases in which the congestion is limited to the pituitary membrane or to the smaller intracranial vessels, the condition has not

proved of serious moment. In those cases, however, in which the deeper sinuses are involved, or in which the suppression or cessation of the menses had been too rapid, apoplectic lesions of the brain or organs of vision have been sometimes a complication.

When nasal hemorrhage or congestion has become the precursor of such serious involvement, the adoption of a carefully restricted system of diet, and the enjoining of strict abstinence from undue excitement, have been found to be important helps toward the restoration of the patient's health.

Diarrhea is an affection attendant on the menopause. I have collected the histories of upward of twenty cases; in some instances the symptoms were mild, in others the morbid condition had continued for several years. Ulceration of the rectum and of the sigmoid flexure of the colon had not infrequently been the result. Distention of the intestines, pressure upon other organs, and painful gastric disorders have often characterized the complications met with. In two cases cancer of the rectum followed, one at the end of the fifth and the other during the middle of the sixth year after the occurrence of the menstrual cessation and the supervention of diarrheal disease.

Leucorrhea and post-menstrual catarrh have been among the most common class of affections incident to perversion of function for which patients have sought relief. When such disorders have been dependent upon local lesions, early curettement, removal of growths, and repair of tissue have frequently been followed with speedy and permanent cure.

As before remarked, the cessation of the menses has been productive of psychical changes. This alteration or loss in mental power has been made manifest by more frequent display of irritability of temper, by the occurrence of hysterical attacks, and occasionally by those of melancholia. Change in the physique, increase of fat, and growth of hair upon the chin and face are not infrequently observed. Pruritus vulvæ and cutaneous eruptions which appear or become aggravated are sources of much annoyance. They arise for the most part from local neuroses or from peripheral neuritis. Increase of sexual activity that may take place is, according to my observation, of a limited or an evanescent manifestation. Rigidity or spasm of the recti muscles, due to tympanites or to gastro-intestinal irritation, presenting the semblance of pregnancy, may readily be diagnosticated by the employment of an anesthetic. Uterine fungosities and polypoid development are frequently the

result of endometritis or uterine engorgement, and may be overcome by early curettement and the application of local styptics. Fibroid growths, which not infrequently appear during the earlier portions of the menstrual period, are probably superinduced by a preternatural development of the stromal over that of the glandular element, for the earliest traces of the sexual organs, according to Haeckel, are visible in the embryo at the point where the skin fibrous layer and the intestinal fibrous layer meet.

After the period of the menopause has arrived such neoplasms may of course be arrested in their growth, on account of the regularly recurring vascular medium being withheld or diminished. It is during this new, altered, or lowered condition of vitality of woman that cancerous, sarcomatous, or other degenerative changes of tissue are assumed. The liability to the occurrence of sequelæ of such a grave nature necessitates the exercise of much skill for their recognition and the adoption of decided measures of treatment for their eradication.

Early cessation of the menses may be superinduced by excessive involution of the uterus and by atrophy of the ovaries consequent on the presence of peculiar factors during parturition. The more gradually the function of the menses terminates the milder will be the local and general disturbances. In those cases in which there is marked impairment of the health at or during the climacteric, serious pathological changes in the pelvic or abdominal organs will doubtless be found to have taken place.

At the menopause the ovaries present a furrowed and shrunken appearance from the periodical separation and discharge of the ova. In women of advanced life the ovary may appear as a mere rudiment. The vaginal introitus may become so constricted as to lose all semblance of its former size and to present in its shrunken, conical form scarcely an opening for entrance. The external cervix may close and thus give rise to an accumulation of the products of catarrh; the collection may be of the character of what is termed hydrometra or physometra, as the inclosed cavity embraces a liquid secretion or one of a gaseous formation. In one case the mass of fluid found in the cavity of the uterus between the fundus and the internal cervix was so firmly held that it could be evacuated only by resort to free dilatation and curettement of the enlarged cavity above the inner cervix. In another case the accumulation, occurring between the external and internal cervix, together with an imprisoned collection above the internal constriction (uterus bicam-

eratus vetularum), was finally overcome by frequently repeated dilatation of the uterine canal. Retention of the form and size of the mammæ, due to increase of fat after the glandular tissue has been more or less absorbed, can be recognized by noting in examination the absence of the characteristic lobular, glandular feel which is peculiar to the normal, active mammæ.

In some cases it is not altogether easy to determine exactly the line of demarcation that separates the borders of the physiologic from the pathologic processes. It may be said, however, that in those cases in which no pelvic lesions exist and no effects of traumatism of parturition remain, the menopause will entail comparatively little danger, provided the change is not hastened by subjective influences and external factors. The simplest and most common of all symptoms that are experienced, as before related, are the hot flushes, the attacks of vertigo, abnormal sensations in the abdomen, in the extremities, and in the breasts; special areas of the nervous system become irregularly congested or irritated. These may be intensified by psychical influences, such as shock, fright, or the effects of mental emotion. The onset of a severe or septic malady, as a pelvic, an abdominal, or a constitutional affection, may most seriously complicate the condition.

Much that has been mentioned will depend upon an hereditary disposition and a proneness to disease. It must be conceded that the closing of the function of the sexual activity, especially in those who have been married and who have been the subjects of parturient action, marks a most important epoch in the physical and psychical processes of the individual.

THE GONORRHEAL PUERPERIUM.

By CHARLES GREENE CUMSTON M.D.,
BOSTON.

In days gone by, severe diseases which had as characteristics high fever and a dulness of the special senses were designated under the general head of typhoid fever; but clinicians have, little by little, separated from the general typhoid state single pathologic processes, such as pneumonia, meningitis, etc., and at the present typhoid fever is a well-defined disease. This result was only attained by carefully recording the different descriptions, and finally a differential diagnosis between the various infectious diseases was made out. Then, with the appearance of the science of bacteriology and with its rapidly perfected technique, endeavor was made to ascertain if each infectious disease had its own special micro-organism, which could be considered as its exclusive factor, and in many cases this result has been attained. Bacteriologists have also done their share in subdividing infectious processes that were formerly thought to be due to a single agent, as, for example, the pneumonia due to streptococci and the pneumonia due to the pneumococcus.

In much the same fashion puerperal sepsis was formerly considered as a single process, always accompanied with a more or less high degree of temperature. The older gynecologists and obstetricians unfortunately had at their disposal a large number of these cases, with a frightfully high mortality, as compared with the present statistics since the introduction of antiseptic methods. It was not long before surgeons were able to distinguish certains forms of puerperal septicemia, and soon a number of varieties were described. Considering the entire course of this affection, a distinction was made between the acute processes which affect the entire organism, and the lighter infections which are limited, at least in the beginning, to the uterus. According to the manner in which extension of the infection took place, either by the bloodvessels or the lymphatics, the processes termed septicemia and pyemia were

defined, while ulcerative endocarditis and phlegmasia alba dolens were also included in this group. For the localized septic processes names were selected in accordance with the location of the infection and have been designated as puerperal vulvitis, colpitis, endometritis, parametritis, and perimetritis.

Ignaz Semmelweiss, in 1847, was the first to open up the road for the exploration of the etiology of these divisions, which, from the differential diagnostic point of view, are very distinct. Semmelweiss repudiated the theory of cosmic or telluric influences on the origin of septic puerperal processes, and upheld that for any type of puerperal infection decomposed organic matter was the true factor. He, however, admitted two manners of infection—namely, direct infection of the patient by hands or instruments, and, secondly, autoinfection. By autoinfection he inferred that a poisonous material could be elaborated in the puerperal uterus and could spontaneously originate there. As is well known, the teachings of this great man met with much adverse criticism in the beginning, and it was only after Lister had demonstrated the practice of antisepsis that his ideas became generally accepted.

Bacteriological science has done its best to supply bacteriological proofs of these clinical experiments and observations; it has searched in vain for a single specific germ for all diseases of the puerperium, but, with all the great amount of research, it has been uniformly proved that the pus-producing organisms—namely, the streptococcus and the staphylococcus—are the factors of puerperal fever.

When Goldscheider wrote his important paper a few years ago, all severe cases of puerperal septicemia were ascribed solely to the streptococcus, and he admitted that the staphylococcus was in play only when the process was distinctly limited; Winckel sustained this teaching.

The number of bacteria producing the less severe types of puerperal septicemia is far greater, and all authorities who have clinically or bacteriologically studied the factors of puerperal infection now admit a fundamental difference between infection, the active entering of the bacteria into the living tissue; autointoxication, which is produced if bits of placenta or membrane are retained in the birth canal and there decompose; and the entrance of the toxins of these saprophytes into the general circulation. Spiegelberg was the first to point out the last condition, and Duncan named it sapremia.

The principal characteristic of sapremia is that, when the noxious

cause is removed, the process is cut short if done in time. In the localized forms of puerperal sepsis the streptococcus, as well as the staphylococcus, may also be in play as in the more serious infections; and Hartman, Morax and many others, including the writer, have been able to demonstrate the presence of the bacterium coli commune in intraperitoneal abscesses arising during the puerperium.

Bumm has pointed out that the streptococcus, as well as the staphylococcus, is only pathogenic facultatively, and this is a well-known character of the bacterium coli. Bumm is of the opinion that the severeness of the infection is in direct relation to the condition of the soil, as well as by the degree of virulence of the pyogenic organism present; and he has asserted that there must necessarily exist a third factor, which is probably a chemical matter, which renders the tissues less resistant to the penetration of the bacteria when once they have gained access, but as yet this chemical substance has not been demonstrated. Besides the question as to when and how pyogenic bacteria become pathogenic in the puerperal state, we have also to consider the question of autoinfection.

It has often been noted that after confinement in which neither manual nor instrumental interference has been employed, the patients have run a temperature, and in order to explain this phenomenon certain writers have again resorted to the theory of autoinfection as emitted by Semmelweiss. Ahlfeld has most emphatically asserted that pyogenic bacteria might be present in the genital organs before labor, and that when this had taken place the result would naturally be a postpartum infection.

When this theory was emitted a very careful and minute technique of disinfection was introduced. "Microbes are all about" was the cry, and in the heat of the struggle against bacteria it was entirely forgotten that a normal labor was a physiological process, whose termination, if left untouched, would in all probability result in the rapid recovery of the patient. It is certainly very droll when we recollect that only thirty years ago the fanatics on antisepsis went so far as to demand the draining of the puerperal uterus in order to prevent the lochia from becoming infected; but very soon statistics showed that with all this complicated technique no decline in the mortality of labor cases occurred, and consequently at the present time the exaggerated processes of disinfection in normal labor have been practically abandoned.

The technique of cultures has so greatly been perfected in the last few years that the question of autoinfection has been pretty thor-

oughly studied with the aid of bacteriology. Döderlein, Krönig, Stroganoff, Menge, Winter and many others have examined the secretions of the genital tract during pregnancy, labor, and the puerperium, from the vulva to the endometrium, but their results are as yet most contradictory on a great many points. The only fact that seems to be pretty clearly established is that from the internal os uteri upward the genital organs are free from bacteria, while microorganisms are frequently present in the lower parts, a number of them being, however, a certain type of long bacilli producing an acid reaction of the discharges, and which are harmless.

The theory of autoinfection, which at present is neither entirely abandoned nor thoroughly believed in, has certainly enriched the etiology of puerperal fever, inasmuch as in certain cases of cryptogenic affections arising during the puerperium it became evident that other influences which are in no way connected with the confinement may be the means of a complication during the puerperium, and that under certain circumstances certain ones of the acute infectious diseases will produce pyrexia during the puerperium; but if a subject is attacked by one of these infectious diseases we cannot correctly say that we are dealing with a puerperal fever, but, more correctly, *a fever during the puerperium.*

One of the earliest infections described as a complication of the puerperium was erysipelas, but it is as yet doubtful whether or not the two types of streptococci—namely, the streptococcus pyogenes aureus and the streptococcus erysipelatis of Fehleisen—are identical. Winckel believes in the identity of the two forms, while Gusserow believes that they are related to each other but differ in their effects. Olshausen was one of the first to call attention to the relationship between scarlet fever and puerperal septicemia. Ballantyne has described a case of hematoma of the broad ligament produced by scarlatina infection, while Werth found Eberth's bacillus in an abscess of the ovary. The influence of measles on the puerperium has been well described by both Klotz and Ballantyne. The latter has reported a case of premature labor where the patient showed many spots of the characteristic eruption of measles. Bumm has found Löffler's bacilli in the endometrium, while Ripperger has demonstrated that influenza may disturb most seriously the normal course of the puerperium. Massen has conclusively proved that all infectious diseases show their presence in the genital organs of a female in the form of an interstitial hemorrhagic endometritis, and in 1895, in a series of clinical lectures on "Metritis as the Cause of

Miscarriage," the writer has fully gone over the ground of this subject.

Of all the acute infectious diseases, it is certainly gonorrhea which plays the most important part in the complications arising during the puerperium. Owing to the fact that the acute stage of this affection is not of long standing in the male, and that gonorrhea seldom extends beyond the posterior urethra, and that the appearance of gonorrheal metastases, generally speaking, is by no means of frequent occurrence, and up to a short time ago gonorrheal infection in the male was considered cured when the subjective symptoms had disappeared, it is not to be wondered at that only a very few years ago the disease, when occurring in the female, was considered as a mild affection localized in the vagina.

It is perfectly natural that Noeggerath, for purely clinical reasons, became convinced that the sexual life of woman could be most fatally interfered with if gonorrheal infection took place, and in the beginning he found but very few believers in the proofs which he adduced to uphold this theory; but at the present time his original writings form the basis of recent researches on chronic gonorrhea of the female. Noeggerath not only emphasized the extremely frequent occurrence of chronic gonorrhea in the male, which he estimated to amount to 80 per cent. of the male population in large cities, but he asserted that 90 per cent of cases of chronic gonorrhea were never cured, and that the gonococci present without producing symptoms were present in a latent form, as he expressed it, and as such were capable of infecting the female. The consequence was that of 100 women whose husbands had had gonorrhea at some time in their bachelor life, there were scarcely 10 that were well, and one might assume also of that tenth part that at some time or other gonorrhea would be contracted. Basing his assertions on 50 cases, Noeggerath considers perimetritis as the most frequent and the most severe consequence of chronic gonorrhea.

According to its occurrence he divided perimetritis into four types, namely: (*a*) the acute, (*b*) the recurrent, (*c*) the chronic perimetritis, and (*d*) ovaritis. According to this observer perimetritis most frequently arises at about the third week postpartum, while the greatest amount of damage inflicted by this process was complete sterility in more than 50 per cent. of cases.

Fritsch was the first who undertook to refute some of Noeggerath's assertions, and he chiefly was opposed to the theory of "latency" as put forth by Noeggerath, inasmuch as "an affection of the body is

something actual and does not cling like a curse to the organism." Noeggerath's second assertion — namely, that gonorrhea always attacks the entire genital tract—Fritsch tried to refute by citing some of his clinical experiments. In one of his cases, however, in which a perimetritis occurred after miscarriage, the etiological factor of which could only be considered as the gonococcus, the patient recovered after a protracted illness. He nevertheless asserts that gonorrheal perimetritis is far from being as frequent as Noeggerath would have it.

About ten years later Fritsch again disputed the correctness of Noeggerath's theories regarding sterility following gonorrhea. Winckel and Olshausen both stated their conviction that gonorrhea is a much more serious affection than was formerly assumed, but at the same time they insist that the theories regarding sterility are exaggerated, and Winckel expressed himself by saying that if Noeggerath's theory held good to its full extent, mankind would be on the road to destruction. Schröder agrees entirely with Noeggerath in his treatise written in 1887, although at the beginning he was inclined to contradict the former's opinion. He only deviated from his views in the part relating to the frequency of the infection, and Noeggerath himself considerably reduced his percentage later on. The most decided adherent to Noeggerath was Sänger, and in a paper published some years ago this authority stated that one-ninth of all diseases of the female genital organs were due to gonorrhea. Fifty per cent. of these patients presented lesions of the tubes and peritoneum which had resulted from an ascending infection during the puerperium, and from this he concluded that almost all suppurative lesions of the adnexa are chiefly gonorrheal in nature. According to Schwartz the percentage of gynecological cases afflicted with gonorrhea may be put at 12.4 per cent., his diagnosis being partly clinical and partly by smear cultures. He examined 617 females, 112 of whom presented gonorrhea, and, like Sänger, he points out the frequency with which the tubes become involved from an ascending infection, and predicts that in the future many cases of ovarian abscess, pelvic peritonitis, and generalized peritonitis will prove to be due to the gonococcus.

Some time later Sänger made the statement before the first Congress of German Gynecologists that one-eighth of gynecological affections were of a gonorrheal nature; and in order to get a correct idea of how often complications during the puerperium are due to gonorrhea, he examined the gynecological records of the Leipsic

clinic and found that 26 per cent. of the pregnant women were affected with this pathologic process. In order to corroborate this statement he referred to Oppenheimer, who a year previously had studied the cases in the gynecologic clinic of Heidelberg, and who had found that 27 per cent. of the pregnant women there admitted were afflicted with gonorrhea and that 40 per cent. of the children born in the institution presented a gonorrheal ophthalmia. That this latter affection could never be produced by normal lochia has been demonstrated by Zweifel, who inoculated the conjunctiva with healthy lochial discharges and always with a negative result. Lomer, who at that time was connected with Schröder's clinic, had also made researches for the gonococcus in 32 pregnant women and found the organism present in 9 of them, or, in other words, 28 per cent.

It must be stated that Sänger made his diagnosis from a purely clinical stand-point, as he did not consider bacteriologic examinations reliable, and he also pointed out the importance of gonorrheal ophthalmia as a diagnostic point of gonorrhea in women recently confined.

From these figures it at once becomes evident that more than twenty-five per cent. of pregnant women are afflicted with gonorrhea, while at the same time the mortality during the puerperium is comparatively small, and Sänger concluded that gonorrheal infection complicating the puerperium can occur in the two following manners, namely: (1) To an existing gonorrhea there is added a septic infection, which he terms a puerperal-gonorrheal mixed infection; (2) gonorrheal infection which ascends during the puerperium, in which case the disease is due entirely to the gonococcus. This simple infection may have produced an endometritis during pregnancy without having interfered with its progress, and the proof for this assertion he bases on a case reported by Donat, where a miscarriage took place only at the eighth month, although there were numerous small foci of suppuration present in the maternal and fetal choria.

Clinically, Sänger divides puerperal gonorrhea into a process developing *soon after* labor, and, secondly, a process developing at a *later period of the puerperium*, and he gives an example of each. The early infection occurred in a primipara who contracted an acute gonorrhea from her husband nine days after labor had taken place. Three days after coitus she developed a pelviperitonitis, from which she recovered at the end of twenty-two weeks. The example of a late gonorrheal infection was demonstrated in the case of a patient

who had given birth to her sixth child; during the third week of the puerperium she developed peritoneal symptoms, which, however, were milder in their manifestations than those met with in a true septic peritonitis. Examination showed a deep laceration of the cervix and a parametritic exudate; the right adnexa had been affected ever since the first labor. On account of the pain present, Sänger removed the diseased structures later on and found that the left tube and ovary were normal, while the right tube had been transformed into a pyosalpinx of fairly good size. This patient's husband was affected with both gonorrhea and stricture. Sänger doubts whether gonorrhea shows itself so early frequently, and thinks that the acute peritoneal stage of the affection usually begins during the second or third week of the puerperium, or even later. In both cases the inflammatory process extended beyond the tubes to the peritoneum; in the case of the early infection a fresh inoculation of the gonococcus was necessary, while when the disease shows itself at a later period Sänger believes that it arises spontaneously from a gonorrheal focus previously present. He states that the extreme tenacity of the gonococcus is very evident from the simple fact that it persists long after the puerperium, although a large number of various organisms that are present in the genital canal die off, but Neisser's organism persists, in spite of the elimination of the endometrium, without being destroyed or being expelled. When the regenerated mucosa has developed it will grow more luxuriantly on it than before.

Graefe has recorded another instance of early infection which very closely resembles the example reported by Sänger. A woman who had been infected two days before her labor by her husband, who had an acute gonorrhea, became ill twelve hours after labor with symptoms of severe septic infection. The condition then changed in a most extraordinary manner: the temperature, which had attained 40° C., rapidly declined, but the pulse remained small and very rapid; the patient looked as if she had an attack of cholera, but ice and opium produced a rapid recovery. On the second day after birth the child developed a gonorrheal ophthalmia.

As in the case of septic puerperal salpingitis, which may result in fatal peritonitis from a rupture of the tube and escape of pus into the peritoneal cavity, so can gonorrheal peritonitis originate from a previously existing gonorrheal infection of the tubes; and Sänger believes that a certain time is required during which gonorrheal pus can extend from the endometrium into the tubes, and from there

infect the peritoneum, and that in this manner we can explain the occurrence of a late infection during the puerperium. Sänger agrees with Noeggerath that gonorrheal peritonitis is always limited to the true pelvis and never becomes generalized.

Sänger's views led Kröner to examine the question. He saw the mothers of children that had been treated in the Breslau clinic for ophthalmia, obtained the history of the mothers, and made a local examination. Out of 91 women that he examined, 35 positively stated that they had had fever during their puerperium; but he, however, doubts that the trouble was caused by gonorrhea in all of them, and only admits the diagnosis of gonorrheal infection when the specific organism has been demonstrated to exist in the genital organs. On the other hand, Sänger insists that his method of investigation was much better, and he stated that he had found 35 cases out of 230 cases of gonorrheal infection in which the disease could be traced back to the confinement itself. The return of menstruation and sexual life; the rapid spreading of the process in the regenerated endometrium, which has become more receptive; the more patent condition of the uterine orifices of the tubes, and subinvolution of the uterus, are all adduced by Sänger for the explanation of the occurrence of tardy gonorrheal infection.

The bacteriologic demonstration as to the nature of the gonococcus is based upon the results of the researches of Bumm, which he published in 1895. By using human blood serum as a culture medium, Bumm was able to demonstrate various types of diplococci closely resembling Neisser's organism, both in their appearance and in staining, but which when inoculated he found were not pathogenic for the male urethra, and he termed them pseudogonococci. Neisser's organism is distinguished from the pseudo types by the fact that it actively penetrates into the protoplasm of the leucocytes and is found situated around the nucleus of the cell. By means of a pure culture Bumm was able to produce gonorrhea in a healthy urethra, and was thus able to comply with the third condition of Koch—namely, the proof of the specific action of a micro-organism demonstrated by a successful inoculation of a pure culture.

From examination of sections made from the human eye, Bumm concluded that the gonococcus would neither penetrate the pavement epithelium nor the stratified pavement epithelium, and much less the connective tissue, nor did it act as a factor of inflammation, and he concluded that the only surfaces on which it would grow were the mucous membranes, which are covered with a cylindrical epithelium.

It is true that Bumm had been forced to admit that the gonococcus might set up lesions in the pavement epithelium in young people, and consequently one of the principal dogmas of his teaching became practically valueless; but, nevertheless, basing his theories on his personal experiments with pure cultures of the organism, he adhered to the idea that suppuration in the connective tissue, or in any other form of epithelium excepting the cylindrical type, could not be produced by the gonococcus alone, but only by a mixed infection with either the streptococcus or the staphylococcus. Bumm and Gerheim consequently upheld that there was no pure gonorrheal colpitis, peritonitis, arthritis, or abscess of the ovary; but, contrary to their assertions, a good many observations have been reported which demonstrate the ease with which the gonococcus may produce an inflammatory process in all types of epithelium as well as the connective tissue.

Touton and Jadassohn demonstrated this fact in the male urethra, while Fritsch showed that the connective tissue of the mucosa of the rectum could be penetrated by the gonococcus up to the muscular fibre. Dinkler proved the same fact in cases of gonorrheal conjunctivitis, while Deutschmann demonstrated the very important fact that a true gonorrheal peritonitis might and did occur; this latter lesion was especially important, because the epithelium of the peritoneum is a very similar structure to that lining the synovial capsules of the joints. Menge and Zweifel pointed out that the frequent occurrence of a former inflammatory process in the peritoneum, arising from a similar lesion in the adnexa, and where the gonococcus could be demonstrated in the pus obtained from the tubes, decidedly diminished the value of Bumm's assertion that the gonococcus could only thrive on a cylindrical epithelium. Zweifel also demonstrated the presence of the gonococcus in the pus of an ovarian abscess, and Sänger has reported similar cases.

Menge emitted the following conclusions from his researches: 1. It has been proved that the gonococcus can penetrate the pavement epithelium. 2. The gonococcus is a true pyogenic organism and can produce peritonitis. 3. The fact that the gonococcus cannot be found in a peritoneal exudate cannot be regarded as a proof that it is not the etiological factor of the process, any more than can the escape of gonorrheal pus into the abdominal cavity without any resulting inflammatory process taking place; for not only would the laparatomy be carried out antiseptically and thus avoid infection, but the pus, having remained for a long time pent

up in a pyosalpinx, would have lost its virulence considerably or entirely.

More light was thrown on the bacteriologic side of the question, as well as clinical experiments on gonorrhea in the female, by Wertheim, who first obtained cultures of the specific organism by direct inoculation of pus on Petri's plates. He more especially demonstrated that the gonococcus is to be distinguished from other forms of diplococci, from the fact that the organism of Neisser penetrates the protoplasm of the pus corpuscles and is decolorized by Gram's method. The gonococcus can be easily grown by the plate culture method, if human blood serum, to which agar-agar has been added, has been used for a culture medium, and in three days a very good culture of gonococcus will have grown. The pure cultures obtained by means of the plate method would give rise to a true gonorrhea when inoculated in the urethra. Human blood serum is by far the best culture medium, but a growth of the organism may be obtained on agar-agar, although it will be poor. The colonies on a plate culture have an irregular shape; their color is brownish yellow. In contrast with the staphylococcus the latter have a much more homogeneous form, and the streptococcus which very soon forms chains. Well-developed cultures of the gonococcus may be reinoculated on fresh blood serum in four or five weeks and a new growth will be obtained. The virulence is not rapidly lost by cultures on artificial media, and a colony which had been present six weeks on human blood serum has proved to be perfectly virulent when introduced into the male urethra.

In a pyosalpinx of three months' duration, in which the abdominal ostium had become obliterated, very virulent gonococci were present in a pure culture. In this very interesting case Wertheim was also able to demonstrate gonococci in the submucous connective tissue of the tubes, in the pus of the abscesses of the ovary, as well as in the subserous tissue of the peritoneum and in the fibrinous membrane covering it. From this he concluded that the inflammatory process had spread in the connective tissue of the broad ligament to the ovary, and he also concluded therefrom that an ascending gonorrhea was a uniform process, each one of whose stages could be produced by the gonococcus without any mixed or secondary infection taking place, even in those cases where the lesions were very far advanced. Wertheim coincides with Bumm in the opinion that gonorrheal peritonitis forms a locally limited affection and never produces a generalized peritonitis, as does septic puerperal peritonitis. To study the

effect of the gonococcus in the connective tissue, Wertheim followed Bumm's method—namely, to inject subcutaneously on his own person pure cultures of the gonococcus—but, unlike the results obtained by Bumm, he was able to produce an inflammatory reaction of short duration.

In another series of experiments supplementing these results, Wertheim took up the question more especially as to how often pus-tubes are due to gonorrhea. Out of 90 cases he found the pus was sterile in 51 instances, while in 49 cases it contained bacteria; in 25 of the 49 cases the gonococcus in pure cultures was obtained from the pus in the tubes—in other words, more than 50 per cent.

Witte spoke very decidedly in favor of the possibility and frequency of a mixed infection as a result of his researches, and, carrying these on, he found gonococci present 4 times out of 24 positive cases, once in a pure culture and 3 times in combination with other bacteria. In 52 cases of pus-tubes examined by Prochownik, 26 presented the evidence of the presence of bacteria, while the gonococcus was only obtained in 2 specimens, once in a pure culture and once combined with the streptococcus.

Whether or not Wertheim was right as to his percentage of cases in which the gonococcus can be demonstrated, he is at any rate the first who showed its presence in the female genital tract by staining the secretions and cultivating it, and he has pointed out the immense part played by this organism as a factor of pyosalpinx and circumscribed peritonitis without the aid of other pyogenic bacteria. His inoculations also permanently refuted the old belief that an infection with chronic gonorrhea could only produce a chronic gonorrhea, because he was able to obtain from long-standing discharges very virulent cultures, which, when inoculated on a healthy urethra, would give rise to an acute process; and this also definitely settled the question of Noeggerath's latent gonorrhea, which is nothing else but a latent lesion containing virulent gonococci. All this also went far in demonstrating the true pathology of puerperal gonorrhea.

Oppenheim was fully aware that the specific organism could be found in very large numbers in the lochia of females affected with gonorrhea, and that this liquid was a very excellent culture medium. Sänger and Lomer came to the same conclusion, while Bumm found large quantities of gonococci in the lochia from the second to the fifth day and after, either contained within the leucocytes or in clusters between them. The gonococci found were very large, which

caused Bumm to believe that the puerperal process was extremely favorable for the development and growth of Neisser's organism. At this period the lochia contained hardly any other organisms; in fact any other types were entirely lacking. The process and evolution of the lochial secretion varies, but not infrequently the secretion stops about the end of the third week of the puerperium and is only present in the form of a slight mucus discharge, and it is just at this period that the presence of the gonococcus is difficult to demonstrate.

Von Steinbüchel undertook a series of experiments at Chrobak's clinic in order to examine the views of Noeggerath and Sänger. He, in the first place, ascertained the number of pregnant women who were suffering from gonorrhea by carefully going into the history of the cases and by bacteriologically confirming his examinations, and at the same time he made the same observation that Bumm had made previously—namely, that the secretion of the pregnant women suffering from gonorrhea did not differ microscopically from that of other pregnant women, either in quantity or quality. He examined 318 women, 70 of whom were affected with gonorrhea—in other words, 21 per cent. Bacteriologically, proof of the presence of gonorrhea was only absolutely positive in 22 out of the 70 cases. As a clinical proof of the presence of this affection, Von Steinbüchel points out that condylomata, cervical catarrh without erosions, and granular colpitis are symptomatic; Sänger's assertion that these three affections are pathognomonic has long since been disproved. It is a well-known fact that condylomata acuminata may also be due to other causes, as, for example, a considerable moisture of the parts, and it is of frequent occurrence to meet with a granular colpitis in women who are not pregnant. As far as cervical endometritis is concerned, it may simply be said that pus from a gonorrheal process will destroy the mucosa of the cervix just like any other form of pus. If we consider these sources of possible error, the statistics offered by Von Steinbüchel must be considered as representing too great a percentage; but, nevertheless, the number of pregnant women affected with gonorrhea is still extremely high. He considers with Fehling that a normal puerperium is one in which during the first week the temperature does not rise above 38° C.

Out of 68 cases of gonorrhea (excluding accidental causes, such as tuberculosis, erysipelas, pneumonia, constipation, etc.), 7 presented fever in the early part of the puerperium, in 4 it was present for more than one day, while in 3 it was only present twenty-four hours.

Out of 242 patients who had been delivered and who were free from gonorrhea, 21 had a rise of temperature without any accidental cause being present. Comparing the 9.3 per cent. of gonorrheal fever with the 9 per cent. of puerperal fever due to other sources, the above-mentioned author believes that he is entitled to conclude that the influence of gonorrheal infection on the early stages of the puerperium is practically *nil;* but, on the other hand, he confirms the opinion of Sänger and Bumm—namely, that in the late stages of the puerperium gonorrheal symptoms are very frequent. He ascertained the ultimate condition of 8 pregnant women who presented gonorrhea before their confinement, and found that 6 of them were taken ill, from two to three months after their labor, with perimetritis. One of the patients presented the first attack of peritonitis as early as the twelfth day postpartum; she was a prostitute in whom an abortion had taken place during the fourth month of gestation, and during the puerperium large numbers of gonococci were present in the secretion from the urethra, but there were very few present in the lochia. The patient was dismissed on the eighth day. Twelve days after the confinement, presumably on account of too much effort in walking, she was taken with intense pain in both hypogastric regions, and fever and pain in the abdomen when pressure was used. The diagnosis at that time was peritonitis, and after remaining in bed a fortnight she recovered, and since has remained perfectly well. Examination five months after labor revealed nothing abnormal in the right adnexa other than a few perimetritic adhesions extending to the pelvic wall and which bound down the tube and ovary quite firmly. The left tube had grown adherent to the ovary and was embedded in a mass of perimetritic exudate, so that they formed an immovable tumor the size of a large walnut. Von Steinbüchel concludes that the late appearance of gonorrhea during the puerperium may be attributed to the distance which the gonococcus must travel to reach from the internal os to the uterine opening of the tube, and also to the fact that the lochia have a tendency to wash away the organisms, thus rendering their ascension difficult; but when the lochia decrease in amount this source of hinderance to reaching the tube is done away with.

The conclusions of Von Steinbüchel are contradicted by Krönig as far as they concern the early part of the puerperium. This latter observer was able to find the gonococcus in 9 women who had been delivered and who were suspected of having gonorrhea, the lochia having been obtained from the uterus by means of sterilized

glass tubes. It is only by this method that it is possible to obtain lochia from the upper genital tract that have not become mixed with the secretion from the lower part of the tract. Krönig made cultures according to Wertheim's technique and was able to obtain pure cultures of the gonococcus, and he also demonstrated their frequent occurrence in the lochia, which microscopically showed that almost every cell contained the organism. Only 1 of these 9 patients was free from fever, while all the others showed an elevation in the temperature varying between 38.5° C. and 40° C. In all these cases the fever subsided without any treatment, and there was no other etiological factor to account for the elevation of the temperature, and two of the patients had been delivered without a single digital examination. In all of these cases the lochia were greatly increased, but did not have any odor. In six of the patients the temperature remained normal after the end of the first week, but the lochia, nevertheless, continued very profuse. In two instances the symptoms continued into the latter part of the puerperium; one patient presented a pelviperitonitic exudate in the third week, while the other developed a parametritic exudate the size of a hen's egg, which was complicated by a tenosynovitis of the right hand. The pus obtained from the sheath of the tendons gave a negative bacteriologic result.

Krönig concludes that gonorrheal endometritis is in itself quite sufficient to produce a rise in temperature during the early part of the puerperium, and that later on the gonorrheal process may extend upward and produce perimetritic lesions. Whether or not the parametritis occurring in the latter part of the puerperium is due to a secondary infection he is unable to say, but he inclines to the opinion that it is not.

In the discussion following the reading of this paper, Schmorl upheld that cases of infection occurring during the latter part of the puerperium are due to a secondary infection from the streptococcus or the staphylococcus, and he cited three cases of a generalized pyemia following acute gonorrhea, and in all three the pus obtained from the metastases contained only the streptococcus. Sänger and Döderlein are also in favor of a mixed infection with pyogenic bacteria in those cases where pyemic symptoms arise after the presence of gonorrhea has been ascertained.

Another case of a rise in temperature during the early part of the puerperium is reported by Von Franqué; the temperature rose on the third day and lasted for three days. Microscopically, gonococci

were found in the secretions, and also some short rods which could not be grown on any media. The child of this patient presented a gonorrheal ophthalmia on the fifth day. Out of 32 cases of pregnant women who presented a pathological condition of the vaginal secretions, Burkhardt was able to demonstrate the presence of the gonococci in 19. Leopold relates as similar cases two instances of infection during the early part of the puerperium where the rise in temperature took place on the fourth day. The first case was that of a primipara, sixteen years of age, who had a rise in temperature for three days. On the seventh day postpartum a greenish-gray membrane was found covering the vaginal walls and the cervix, which was slightly lacerated. This membrane was examined microscopically, with the result that the gonococcus was found in a pure state. The patient was completely cured at the end of eight days. The second case was that of a female who had given birth to her fourth child and who was admitted to the hospital with a severe cold. The child presented a gonorrheal ophthalmia two days after birth. On the fourth day of the puerperium the patient became feverish and a greenish-gray exudate was found covering the vagina and cervix. Bacteriologic examination of the exudate demonstrated the presence of the gonococcus, staphylococcus, and streptococcus. In this patient the fever remained somewhat longer than in the first case, and the lochia was still in rather large quantities fourteen days after the patient was discharged from the hospital. Concerning this latter case Leopold is of the opinion that the fever was due to a secondary infection of the gonorrheal process from the streptococcus, and as the patient during her confinement had only been examined externally, this would seem to be a fair example of autoinfection.

Krönig was able to demonstrate the presence of the gonococcus in 6 out of 11 patients having a pathologic secretion from the vagina, and 1 of the patients was taken with symptoms of endometritis as early as the second day postpartum. Veit in his writings only alludes to the influence of very recent gonorrheal infection on the process of generation. He relates 5 cases which resemble one another, inasmuch as the infection was caused by an acute gonorrhea in the husband, the wife being infected just before or shortly after labor. In 4 cases the children presented a gonorrheal ophthalmia, and all 5 of the patients presented symptoms of acute peritonitis at about the end of the early part of the puerperium, the peritonitis combining all the classic characteristics of an acute septic type of the disease—namely, severe tympanites and

vomiting, small and frequent pulse, a temperature above 39° C., and intense abdominal pain. The appearance at a later period he considers as one of the most important differential symptoms of gonorrheal peritonitis. In all of his cases the violent symptoms diminished and the patients recovered slowly.

According to Veit the rapid ascension of the affection is due to the relaxed condition of the genital tract shortly before and during the puerperium. In all publications which have appeared, it seems to indicate that the gonorrheal infection, whose presence has been proved by the demonstrations of the specific organism in the lochia, can produce lesions either during the early or the late stage of the puerperium. A definite time cannot be fixed regarding the power of the gonococcus of producing an endometritis of a lighter grade during the early part of the puerperium, and Krönig reports cases in which the gonorrheal fever appeared as early as the second day postpartum ; and in his case, as well as the one reported by Von Franqué, there is nothing in the history that would tend to show that an impure coitus had taken place shortly before or after labor. The process in these cases is to be explained as the upward extension into the endometrium of a preëxisting gonorrheal infection, the process in the endometrium being accompanied by pyrexia of short duration.

In contrast to what has been said we have the reports of Sänger and Veit, which have in common the fact that shortly before or after birth an infection, with a more or less acute gonorrhea, is the factor of the affection ; just how far the part played by the virus of the gonococcus is concerned cannot be definitely ascertained. Wertheim believes that in all cases a gonorrheal process is capable of producing acute manifestations in the subject infected. The symptoms of such forms of the disease probably arise somewhat later—that is to say, only toward the end of the early part of the puerperium—but they then cause very severe peritoneal symptoms, because the gonorrheal process has extended beyond the uterine end of the tube. In contrast to septic puerperal peritonitis, the fever, as well as the severity of the symptoms, soon decreases, and the patient recovers in the gonorrheal type. In both forms, which are consequently different from each other, gonorrheal infection extended beyond the internal os before it influenced the temperature. It is difficult to say how the gonococcus could extend upward during the first few days when the lochia are excreted in such large quantities.

When the endometritis is followed by parametritis with metastases

into the tendinum vaginæ, Krönig does not admit that these secondary complications are due to an infection by the streptococcus, and he consequently entirely agrees with Wertheim that the gonococcus does not merely produce a localized infectious process in the endometrium, but that it also penetrates deeply into the connective tissue, and can then produce a suppurative process. He thinks that the other cases of parametritis may be explained in a similar way if it be admitted that the gonococcus finds its way into the subperitoneal connective tissue through a laceration in the cervix. Continuing this line of thought it might also be assumed that a pyosalpinx could be produced by an invasion of the gonococcus, having its starting-point in the subserous membrane of the tubes and extending through their walls into their lumen, and that, therefore, the endometrium may not be the seat of any gonorrheal infection during the formation of a pus-tube. This theory has generally been admitted as correct since Wertheim's first writings.

Wertheim's conclusions have, however, been considerably modified by Bumm, who admits that the gonococcus grows on all types of epithelium, and that this organism is quite capable of producing either a vaginitis or a peritonitis. Gonococci have also been found in the connective tissue, but Bumm has only found them in the superficial subepithelial strata, and in the various glands connected with the genital apparatus he was only able to find them in the ducts and never in the deeper secreting portions. This result is quite sufficient to explain the pathology of gonorrheal metastases. Wertheim has demonstrated in a case of gonorrheal cystitis a capillary vessel in the superficial submucous tissue, in close proximity to the mucous membrane of the bladder, that was completely filled with gonococci.

The first attempt at inoculating pure cultures of the gonococcus into the connective tissue of the arm gave a negative result, but, as has been stated, Wertheim obtained inflammatory symptoms by these inoculations, while Bumm, carrying on this line of experiments, obtained only negative results. Steinschneider and Richter also obtained negative results.

Wertheim's statement, which is most important from a clinical standpoint, that he had seen the gonococcus produce oöphoritis, salpingitis and peritonitis after it had passed through the connective tissue of the broad ligament, just as occurs in the case of the ordinary pyogenic bacteria, is absolutely denied by Bumm, and he declares that this statement is not supported by the results of any

other bacteriologists, including Charrier. On the contrary, Bumm assumes that in this case a pyosalpinx was present and that the process had extended into the parametrium, and he declares that it is absolutely wrong to uphold that Neisser's organism can produce suppurative processes in the deep connective tissue. He asserts that gonorrheal parametritis is due to a mixed infection; and in a mixed infection with the gonococcus he distinguishes two types—namely, a secondary infection from the staphylococcus or the streptococcus developing after the first manifestations of a purely gonorrheal process; and, secondly, a symbiosis where both kinds of bacteria act at the same time. On the other hand, he admits with Wertheim that a pyosalpinx very frequently has a purely gonorrheal origin, and that the infection may in most cases be traced back to the latter part of the puerperium; and he says that the division of pyosalpinx into a septic puerperal form and a gonorrheal form is no longer sufficient, and it is also necessary to admit a gonorrheal puerperal origin.

In the case of a pyosalpinx which has produced a closing of the abdominal ostium of the tube, an acute or a recurrent perimetritis may be explained, because not only can the encapsulated pus, which is a foreign body, produce irritative symptoms, but, owing to sudden or forced movements, a thin adhesion may be ruptured and allow the pus to escape directly into the peritoneal cavity, after which the rent in the adhesion may become closed. The gonococcus can only set up a suppurative inflammation of the ovary if the pus becomes inoculated on a ruptured Graafian follicle. Bumm also upholds that this form of peritonitis never becomes generalized and never is fatal, and according to him the favorable outcome of these cases is due to the fact that the gonococcus does not find a suitable culture medium on the peritoneum, and consequently leads a short life. The limitation of the peritoneal inflammation is due to the fact that the fibrinous agglutination walls off the process and prevents the extension of the gonococcus to the rest of the abdomen. The material which forms the fibrinous substance, which is termed fibrogenous matter, is contained in the inflammatory exudate, and the fibroplastic substance with its ferment is abundantly supplied by the leucocytes.

Now, while the staphylococcus and the streptococcus do not actively enter within the leucocytes, the gonococcus, contrary to the teachings of Metschnikoff, is able to do so very considerably, and consequently is able to liberate the fibrin ferment. It is for this

reason that numerous adhesions are found in gonorrheal peritonitis and are thrown out with extreme rapidity, while in septic peritonitis due to ordinary pyogenic organisms an abundant exudate is found covering all the viscera.

It may consequently be said that a gonorrheal process may extend upward from the internal os and invade the entire genital tract. Regarding the symptoms, all authorities appear to agree that their course is in every case much milder than in the ordinary septic infection, and the highest degree of a gonorrheal process is represented in a circumscribed peritonitis.

From a careful perusal of a large amount of literature published on the subject in French, German, and English, one thing stands out plainly, and that is that no definite symptomatology or manifestation of a gonorrheal process during the puerperium can be described. Gonorrhea does not appear to produce an elevation of the temperature if the process does not extend above the internal os; Schauta even goes so far as to assert that a gonorrheal catarrh of the cervix may extend to the endometrium without giving rise to any serious symptoms, and Fritsch's paper, which I have already mentioned, certainly points to the correctness of this assertion. In some of the cases recorded in this memoir an endometritis went through its evolution without any elevation of temperature, and an extension of the process from the uterine mucosa to the peritoneum took place insensibly. If a rise in temperature should occur in a case of a pure gonorrheal endometritis, Leopold upholds that it may take place as early as the third day of the puerperium, and therefore we can no longer maintain that a rise of temperature occurring late in the puerperium is a special characteristic of a gonorrheal infection. The acuteness of the progress of the affection in the early puerperium—that is to say, the duration of fever during a gonorrheal endometritis, with or without extension of the process to the tubes or the peritoneum—will in all probability depend upon the virulence of the gonococcus, whether there be a mixed infection or not.

If a newly-married girl infected with an acute gonorrhea from her husband can show a bilateral pyosalpinx at the end of a week—and such cases are of daily occurrence—there is no doubt that a similar process can develop with the same rapidity during the puerperium. Gonorrheal processes occurring late in the puerperium may also be traced to various causes which certainly have a preponderant influence upon the degree and duration of the process. Veit and Winter ascribe the upward extension of the infection to the uterus, tubes,

and peritoneum to the relaxation of the genital organs, or to the return of the menstrual process. Wertheim has expressed a view to the effect that in married people a certain adaptation to the gonococcus takes place, but when sexual intercourse is interrupted the immunity to the organism ceases, and the effect of coitus, when resumed, is a fresh infection.

As far as we can ascertain, all authorities agree that gonorrheal infection during the puerperium runs a milder course than other septic processes arising during the lying-in period. The milder course of the symptoms and their rapid subsidence in puerperal gonorrhea by no means indicate that the process has been cured; and it is a well-established fact that the gonorrheal puerperium is extremely chronic and defies treatment, just as in the case with gonorrheal processes in the female in general. This is especially true of recurrent perimetritis as described by Noeggerath. When the process has involved the adnexa the patient is subject to most severe suffering, and a cure can only be brought about by the removal of the appendages; and in those cases in which the affection is allowed to go on unaided by surgical interference, the patient usually succumbs to a gonorrheal cachexia.

As has already been pointed out, the presence of the gonococcus can usually be demonstrated by an examination of the lochia. The organism produces such varied manifestations in all periods of the puerperium that purely clinical symptoms are not sufficient upon which to base a diagnosis, and it is of absolute necessity to examine the secretions bacteriologically in all cases where gonorrhea is suspected. The tendency of a gonorrheal process to extend upward during the puerperium, and also its persistency to take a chronic course, demonstrate that this disease has a decided influence on fecundation as well as on pregnancy. Acute gonorrhea in the female is in most cases largely limited to the cervix and urethra, and it would seem probable that an acute gonorrheal inflammation of the cervical canal would prevent the male elements from reaching the ovum either within the uterine cavity or in the tube.

On the other hand, a chronic gonorrheal process of the cervix should not interfere with the entrance of the spermatozoa within the uterine cavity, because the cervical secretions are very scant. If the entire extent of the cylindrical epithelium of the endometrium be invaded extensively by the gonococcus, the possibility of an impregnated ovum becoming attached to the uterine mucosa is very improbable. When the endometritis has assumed a chronic form,

in which case the cylindrical epithelium of the endometrium has become regenerated so that only in a few spots pavement epithelium invaded by the gonococcus has remained, pregnancy can take place and go to term, or at least to the seventh or eighth month. If, on the other hand, a gonorrheal endometritis be followed by a metritis, with hypertrophy of the connective tissue and an abnormal development of the uterine glands at the expense of the muscular structure of the uterus, the insufficient elasticity of the uterus will mechanically act against the development of the organ when pregnancy takes place, and the result will naturally be an early miscarriage. This applies to the process whether it originates in the parenchyma of the uterus itself or whether it is due to the gonococcus present in the submucosa.

The most common condition met with is probably that conception and gonorrheal infection are simultaneous or take place shortly after one another, so that the development of the ovum will prevent the upward extension of the gonococcus, and it is only after labor has taken place that the uterine cavity becomes infected.

A pyosalpinx, if present in only one tube, will not interfere with pregnancy, because ovulation will take place on the opposite side and the ovum can reach the uterus through the healthy tube, and this condition is certainly frequently met with in practice. Bilateral pyosalpinx will render the female sterile, but even in cases where both tubes are patent a perimetritis with adhesions will prevent the entrance of the spermatozoa into the uterine cavity, because the uterus is bound down and its position changed in the vast majority of cases. In our opinion, and the same has been upheld by Bumm, the chronic gonorrheal endometritis and salpingitis is an important etiological factor of tubal pregnancy.

The social danger of gonorrhea is its tendency to cause sterility, and Sänger has very ably demonstrated the frequency of what he terms a "one-child sterility" as a consequence of gonorrheal infection. The first pregnancy goes to term, and during or after the puerperium the process extends upward, producing chronic pathologic changes from the internal os upward, and the female thereby loses her aptness of conceiving and carrying her pregnancy to term.

On the other hand, every sterile union must not be attributed to the wife, and out of 205 sterile marriages Fürbringer found that 35 per cent. were due to a former gonorrhea in the husband. According to the statistics made in France by Chervin, out of every 100 marriages 20 were absolutely sterile and 24 gave birth to only one

child. Glünder, basing his researches on the history and partly on clinical examinations, comes to the result that in 84 sterile marriages gonorrheal infection was present 62 times, or, in other words, 77.5 per cent. If on the average 12 per cent. of all marriages are sterile, 8 per cent. of them must be ascribed to gonorrhea; and it seems thus proved that gonorrhea may not only interrupt pregnancy after it has taken place, but may prevent it from occurring altogether.

It is very true that a large percentage of females recover from gonorrheal infection without the slightest trace of the disease remaining, become pregnant and go to term, give life to children who soon after birth develop a gonorrheal ophthalmia, go through their puerperium and lactation without any trouble; while others, from the very receipt of the infection, remain sufferers for the rest of their lives, being attacked by recurrent attacks of pelviperitonitis, have abnormal labors, and often die in consequence of their diseased genital organs.

At the present time an explanation of the above statement cannot be made with certainty, but it would appear to the writer that some inherent weakness of the epithelium, as has been pointed out by Bumm, may account for those cases where the process remains. An infantile development of the female and her genital organs should also be considered as an excellent soil for the development of the gonococcus, as has been pointed out by Freund, and reddish blonde and light blonde females are certainly more severely affected by gonorrheal infection than are darker-complexioned subjects, and here the diathesis of the individual certainly acts as a *locus minoris resistentiæ*.

We will here append five cases of gonorrheal puerperium which we have seen; and although this number could probably be greatly increased, we only report these particular cases because in each instance they were demonstrated bacteriologically to be examples of the disease.

CASE I.—Mrs. A. B., aged twenty-four years, was confined of her first child on April 21, 1895. The labor was comparatively easy and the child was an eight-pound girl. The antecedents of the patient were briefly as follows: She had always menstruated regularly without pain, the flow lasting usually five days. She was married at the age of twenty-one. Three months after marriage the patient noticed a slight glairy discharge from the vagina, which never amounted to much, excepting that it was somewhat increased

during the three or four days preceding and following the menstrual epoch.

Five days after confinement the thermometer suddenly rose to 39° C., pulse 98; there were no chills. The patient also complained of considerable pain in the sacral region and also in the left side. The lochia, which had been normal in amount up to this time, decreased somewhat.

Bimanual examination revealed a small left-sided laceration of the cervix; the uterus was the size of a fetal head, soft and tender on pressure. On the left side could be felt a mass the size of a lemon, which was extremely painful and hard. Nothing could be detected in Douglas's pouch, which was perfectly free. The right adnexa was apparently normal. By the speculum a few erosions were detected on the cervix, which bled rather easily when their surface was wiped over with cotton. There were no apparent lesions of the external genitals, and the urethra seemed to be perfectly normal. An analysis of the urine was also negative. A long platinum loop was introduced into the uterine cavity, and some of the secretion was removed for bacteriologic examination; cover-glass preparations, stained with methylene violet, revealed large numbers of gonococci both between and inside of the epithelial cells. A diagnosis of puerperal gonorrheal endometritis and salpingitis was made. The treatment consisted of intrauterine irrigations twice daily of a 1 : 3000 solution of permanganate of potassium and application of equal parts of mercurial and belladonna ointment to the abdomen.

The temperature fell to normal just one week after commencing this treatment, which was carried out for about four weeks, at the end of which time a bimanual examination showed that perfect involution of the uterus had taken place and that the salpingitis had almost entirely disappeared. Microscopic examination of the secretion from the cervix, taken three weeks after all treatment had been stopped, was entirely negative.

The child never presented any signs of inflammation of the eyes. The husband admitted that he had been a sufferer from gleet for the past nine years, his last attack of gonorrhea having taken place nearly seven years before marriage.

Case II.—Mrs. A. C., aged thirty-one years, seen in consultation with Dr. S. H. Littlefield. The patient had given birth to two healthy children, who were both alive and in excellent health. Seventeen days after the delivery of her third child, the labor being in every way normal, the patient complained of chills in the

afternoon, and in the evening the temperature was found to be 38.7° C., pulse 102, and we were asked to see the patient. She was a well-developed brunette with a negative history. Examination of the thorax was negative. A few enlarged inguinal glands could be detected on the right side. External genitals, urethra and Bartholin's glands were normal. Bimanual examination revealed her uterus about the size of a second month's pregnancy, which was soft and rather tender on pressure. There were a few erosions on the cervix, and a very little mucopurulent discharge came from the os. A platinum needle was introduced into the cervical canal and some of the secretion was removed for examination; a cover-glass preparation, stained with methylene blue, revealed numerous leucocytes containing gonococci, with clusters of the organism between them, and a few epithelial cells were found. The adnexa appeared normal and were not tender on pressure.

A diagnosis of gonorrheal endometritis in the puerperium was made. After complete dilatation of the cervix the uterine cavity was very carefully curetted and then swabbed out with pure carbolic acid. The fever immediately fell, and in three weeks from the time of operation the patient was up and attending to her household duties.

The child never gave evidences of any complication in the eyes. The husband had contracted an acute gonorrhea about a fortnight before the birth of the child; coitus had taken place about ten days after the confinement.

Case III.—Mrs. A. D., aged thirty-five years, was confined of her fourth child by Dr. Rideout, of Somerville, who kindly asked me to see the case in consultation. The genital history of this patient was absolutely negative, and the previous confinements had all been normal. Five days following the confinement the patient had some slight chills and pains in the calf of the right leg. The limb began to swell rapidly, and within twenty-four hours presented all the ordinary signs of a phlegmasia alba dolens. Two days after this the right leg became painful and rapidly developed into the same condition as its fellow. The patient complained of a great deal of tenderness throughout the lower abdomen. We saw the patient on the eighth day after the confinement, and, on account of the condition of the lower limbs, a vaginal examination was difficult to obtain. Palpation of the abdomen revealed a mass extending across the pelvis into both iliac fossæ and upward to about fifteen centimeters above the pubis. The doctor informed us that the child's eyes were very much inflamed on the second day, but by a rigid

treatment with nitrate of silver they were improving. The usual treatment of phlegmasia alba dolens was ordered.

Three days later we were asked to see the patient again, and found that the abdominal pain was becoming more severe and that the temperature was rapidly rising, and on this day had reached 40° C. Ether was given, and the vagina was spread open by two vaginal retractors with much difficulty on account of the condition of the legs. There was a considerable bulging in Douglas's pouch, and bimanual examination revealed a large, diffuse, fluctuating mass behind and on the sides of the uterus. Posterior vaginal colpotomy was done, which gave issue to about 500 cubic centimeters of a thick, yellow-greenish pus which microscopically revealed a considerable number of gonococci both within and between the leucocytes.

To be brief, we will simply say that after five weeks the temperature slowly reached the normal and the phlegmasia alba dolens subsided. Bimanual examination at this time showed that the uterus was somewhat enlarged and in retroversion, bound down and surrounded by a dense mass of adhesions. About nine months later we were obliged to perform a total abdominal hysterectomy on account of the very severe pain and rectal symptoms presented by the patient.

At the time of writing fifteen months have elapsed since the hysterectomy, and the patient is in very fair health.

Case IV.—Mrs. A. E., aged thirty years, was seen in consultation with Dr. Ryan four weeks after having been confined of her second child. The antecedents of the patient, both as to her genital organs and general health, were rather obscure, although there apparently had been no symptoms of gonorrhea. The labor had been normal.

The patient was a slightly built blonde presenting a decidedly lymphatic diathesis. She complained of some pain in the lower abdomen, and for the last two days the right knee-joint had become swollen and painful, and it was for this latter condition that our opinion was asked. Suspecting the true nature of the trouble, a bimanual examination was asked for, which revealed an enlarged and flabby uterus and considerable thickening in the parametrium. Much pain was produced by the examination. Some of the secretion coming from the cervix was removed and showed microscopically a few clusters of gonococci, which were scattered throughout the preparation.

The right knee-joint was considerably enlarged. The local temperature was elevated and the joint was extremely tender, and fluctuation could be elicited. The diagnosis of gonorrheal endometritis and parametritis in puerperio, with metastasis into the right knee-

joint, was made. Irrigations of 1 : 3000 solution of permanganate of potassium were ordered to be given twice daily, and the knee was fixed on a posterior splint, and a 30 per cent. ichthyol ointment was ordered to be freely applied to the joint once daily. Internally fifteen drops of the oil of wintergreen were given four times a day.

The general condition improved under this treatment, and the temperature, which on the day the patient was first seen by the writer was 39.2° C., fell at the end of a week to normal. By the use of the intrauterine irrigations the genital organs were greatly improved, so that eight months after the confinement, when the patient was last seen, a general thickening of the parametrium and a retroverted uterus were all that remained; and except for the rectal symptoms produced by this condition of affairs, as well as some pain at the time of menstruation, the patient was feeling fairly well. The knee-joint was somewhat stiff and presented a certain amount of thickening around the joint, but fairly good movements could be obtained, and the patient was able to walk without much trouble.

The patient's husband was a travelling man, and during his absence from town his wife had sexual relations with a cousin about three weeks before delivery took place.

Case V.—Mrs. A. F., aged twenty-seven years, was delivered of her first child on February 17, 1897. Two weeks after her confinement the patient felt chilly and the temperature suddenly rose to 39° C. The labor had been a tedious one, but instrumental interference had not been necessary. As we only saw this patient once in consultation, we unfortunately do not know the ultimate outcome, but the following local conditions were found. The vagina was lined by a thin, greenish-yellow membrane, which extended up on to the posterior lip of the cervix. The urethra and Bartholin's glands were normal. There were no enlarged glands in the inguinal region. Bimanual examination revealed a somewhat enlarged and tender uterus, with a mass about the size of a walnut on the right side. Examination of the secretion coming from the uterus, as well as of the false membrane lining the vagina, revealed gonococci in considerable numbers. A diagnosis of gonorrheal endometritis, right-sided salpingitis, and vaginitis in puerperio was made, and intrauterine and vaginal irrigations of permanganate of potassium were advised.

The husband admitted that he was suffering from a subacute gonorrhea, which he had contracted about four months before the birth of the child, and that coitus had taken place only on one occasion about six weeks before the confinement.

SUPRAPUBIC CYSTOTOMY FOR THE REMOVAL OF A LARGE ENCYSTED CALCULUS CONTAINING A HAIRPIN.

By L. W. SWOPE, M.D.,
PITTSBURG.

Miss S., aged twenty-two years, a weak-looking and badly nourished woman, much reduced in weight by long and severe suffering from vesical calculus of over one year's standing, came to my office May 24, 1897. Upon inquiry I found from her clinical history that fifteen months before she was troubled with a urethral irritation, and in endeavoring to relieve the intense itching with an ordinary steel wire hairpin, it slipped into the bladder and formed the nucleus for the development of the large phosphatic calculus.

The young woman, being bashful, preferred to suffer, which she says she did bravely, rather than inform her parents of the accident. Finally the time came when her nervous system began to break down from the long and continuous strain; her appetite failed; her weight decreased from one hundred and forty pounds to eighty-five pounds; her urination becoming more and more frequent, and at last dribbling from her as fast as formed. She finally consulted her family physician, who, recognizing her true physical condition and malady, gave her an anesthetic, dilated her urethra, and made an attempt to crush the stone, but failed to do so on account of not being able to grasp the calculus.

On examination I found the urethra dilated to the size of a 36 French sound, and urine, containing pus and mucus streaked with blood, trickling from her. The surrounding soft parts were congested, swollen, and eroded from the discharge, making an internal examination very difficult. Upon introducing my finger into the bladder I came in contact with the calculus extending into the

trigone, and on gently trying to separate the walls of the bladder from the calculus I found it was impossible, as the stone was encysted, and to perform lithotrity under these unfavorable conditions would jeopardize the life of my patient. I sent her to the West Penn Hospital, and ordered her to be prepared for a suprapubic cystotomy.

On May 25th ether was administered by Dr. Keller. I was assisted by Dr. Sanes, senior resident of the West Penn Hospital. The operation was witnessed by my friends Drs. J. W. McFarlane, C. B. King, and R. G. Herron, staff surgeons to the West Penn Hospital. I commenced by attempting to inject a weak solution of boric acid in water into the bladder, but failed, as the bladder would hold only half an ounce. I then introduced the pear-shaped india-rubber bag into the rectum, and distended with twelve ounces of water. The position of the bladder was now very slightly visible above the pubes and could be defined by palpation and percussion. I made an incision of the skin and fatty tissue from the symphysis pubis upward about six inches long, dissecting in the manner directed down to the linea alba. I notched it and incised on a director about five inches. I then divided its connection with the symphysis pubis on each side laterally for about half an inch as well as the attachments of the recti muscles, and separated these muscles with my finger, and continued to divide the transversalis fascia with my finger nail down to the bladder, without any hemorrhage worth noticing.

The hard bladder could be felt by pushing down and back of the symphysis pubis. Inserting a small curved hook, which I held in my left hand, I resumed the scalpel for the purpose of puncturing the bladder sufficiently to allow the index-finger to enter the bladder, which came in contact with the hard calculus. In attempting to make this opening larger the peritoneal cavity was opened. Gauze sponge was used to protect the cavity, and the bladder was divided nearly into halves (antero-posterior) in order to encircle the calculus. The bladder walls were then stripped off the calculus, the mucosa dipping down into the sulci of the calculus, making the operation very slow and tedious, until finally enough room was made for the introduction of a large lithotomy forceps, and after a few attempts I was able to crush the stone and remove it piecemeal. From the conditions revealed I found that my first

impression and diagnosis of the calculus were correct: the stone was completely encysted. The wall of the bladder had lost its tone and become very friable; any slight traction made on its walls would tear out the piece; and had I attempted to force my way into the bladder from below to crush the stone, I certainly would have ruptured the bladder, infected the peritoneal cavity, and lost my patient.

The bladder was washed out with Thiersch's solution to carry off the fragments and broken-down filaments of the mucosa. A portion of the omentum was removed which had come in contact with the infected bladder during the operation. The fragments of stone and hairpin removed weighed a little over eight ounces, and the concretion was phosphatic.

There was no hemorrhage worth mentioning, and no treatment was required in regard to it. The rectal bag was removed. Five sutures were introduced into the upper portion of the incision, in order to close the peritoneal cavity. The remainder of the incision was allowed to heal by granulation. The patient was placed in bed on her back. The urine drained off freely from the wound and the urethra. She stood the operation very badly. Saline injections were given freely during the operation.

Her condition the next few days was very critical; pulse ranged from 110 to 135, temperature from 103° to 104.5°. The bladder was washed out every eight hours with either Thiersch's or permanganate solution under the supervision of Dr. Sanes. Finally the pus discharge began to diminish, the patient began to eat, and her recovery was assured. Four weeks after the operation the pulse and temperature were nearly normal. The tone of the bladder was rapidly being restored to its former condition, the wound nearly healed, almost all the urine being passed by the urethra. She left the hospital on July 6th in very good condition, although weak, but with a good appetite and feeling well. I saw her ten months after the operation; she then weighed one hundred and fifty pounds, did not have to get up at night to void urine, her bladder had perfectly regained its former tone, and she is sound again in all her physical conditions.

ABDOMINAL *vs.* VAGINAL SECTION IN THE TREATMENT OF PELVIC DISEASE.

By O. G. PFAFF, M.D.,
INDIANAPOLIS.

ALL that pelvic surgery is must be accredited to the abdominal incision, and it is a monument to the boldness of those advanced men who first dared to invade the sacred cavity of the peritoneum. For many years following the brilliant pioneer work of McDowell there was no pelvic surgery other than the ligation of pedicles of large tumors, the smaller growths being entirely neglected, owing to the incomplete methods of diagnosis at that time. When the abdomen was opened many supposed tumors were found to be merely cases of abdominal dropsy, and many growths diagnosed as ovarian cysts were shown to be fibroid tumors of the uterus or other solid neoplasms from which even bold operators shrank, so fearful was the mortality when interference was attempted.

Frequently these incomplete operations were regarded as surgical mistakes; but the great injustice of the assumption soon developed the aggressive spirit in the self-respecting surgeon, and the "working diagnosis" and the "exploratory incision" were promptly accorded a position of recognized respectability. With the advent of antiseptic and especially of aseptic methods in surgery the mortality of the exploratory incision became almost *nil*, and the number of such operations was enormously increased, and it is to the conscientious investigations which were only possible through the exploratory incision that we owe our *fin-de-siècle* knowledge of pelvic pathology.

The technique of pelvic surgery through the abdominal incision has in recent years been brought to a very high state of approximate perfection, and yet there remain certain objectionable features which occasionally complicate cases and lead to disastrous results. The objections which one may justly charge are:

1. The impossibility of proper drainage.

2. The exposure of large peritoneal surfaces to the danger of infection in breaking up old adhesions in the effort to reach the pelvic floor, and in dragging septic materials back up through the cavity.

3. The occasional occurrence of hernia.

4. The scar.

A few words in discussion of these four points will be offered later in considering some other facts. The operation known as vaginal hysterectomy, as perfected by A. Martin, was indeed a bold departure and a great triumph of individual courage and rare skill, comparable in vaginal surgery to the achievements of McDowell in the abdomen. At first there were few imitators. The difficulties and risk which it entailed seemed formidable, but the great practical value and wide application of the technique involved soon won for it full recognition and the unqualified indorsement of the profession throughout the world. Equipped with a greatly increased facility of vaginal maneuvering, the result of much practice in the performance of hysterectomy for recognized uterine disease, men have recently attempted to effect through vaginal celiotomy, more or less radical, all that has been heretofore accomplished by the abdominal route, excepting only the removal of large tumors and certain visceral lesions. It is maintained by advocates of the procedure that all the objections which obtain as to the suprapubic method are entirely overcome in the vaginal operation; and as proof of this position, Pryor reports the completion of his *first one hundred operations* without a death. The list includes: "old tubal, ovarian, and peritoneal lesions," tubes distorted and adherent after many attacks of peritonitis, *sixteen times;* pyosalpinx or ovarian abscess, the lesion being in *all cases bilateral, fifty-four times;* diffuse suppuration, *nine times;* pelvic bands, sinuses, or other lesions due to laparatomy, *eight times;* ectopic gestation with pus foci, the sacs sometimes ruptured and sometimes not ruptured, *thirteen times.* In the reflected light of such brilliant showing as the above, it is not surprising that certain enthusiastic surgeons should take the position that laparatomy for pelvic disease should be practically abandoned and only resorted to in case of certain emergencies or unforeseen complications.

It is no doubt unavoidable and in some degree conducive to progress that the elated promulgators of new theories should emphasize their positions by extravagant claims and proceed to advance facts in support of their boldest statements. In this effort the

possibilities are thoroughly exploited, and, in turn, whatsoever is radically different from the accepted attitude of the profession is sure to be earnestly contested, and eventually all of the facts relating to the controversy are made to stand out clearly, and a just appreciation of their true value obtains. I do not believe that operators will display unseemly haste to abandon an old and tried, even if an imperfect, method for a procedure which bears the stamp of novelty and whose merits are only in some degree established, although its advocates be eminently respectable and promise much.

In an effort to reach an unbiased conclusion as to the relative merits of the two procedures as applied to similar cases, and also, if possible, to evolve some rule to guide the operator in making a choice of what is best for his patient, I will present such facts bearing on the case as I can command, many of which are from my own experience, and shall aim to be guided solely by the logic of such facts. The evidence of eminent advocates of either method is of the utmost value and certainly commands our respectful consideration, but shall not be allowed unchallenged to contradict personal experience. I cannot conceive that the technique of vaginal extirpation of adherent tubes and ovaries can ever be made as simple and safe as that of the suprapubic operation. It is certainly not justifiable to separate strong intestinal, and especially appendicial, adhesions in the dark; nor is it justifiable to greatly risk wounding such viscera, even under ocular inspection, unless one is prepared to make immediate repair of such damage, which is utterly impossible when working through the vagina; nor will it do to say that such injuries are rare, and when they do occur we can open the abdomen in the usual way and quickly reach the wounded viscera. Naturally such injuries are rare, but when they do occur, if in the operation by abdominal section, we are immediately aware of such occurrence and at once make the necessary repairs with the minimum of exposure; while if the same damage be done through the vaginal cut, we have to abandon an incomplete operation, begin anew by abdominal section, drag the perforated and leaking gut up through the cavity at considerable risk of infection, and the patient has been subjected to *two* instead of *one* chance of shock and to a duplicate chance of infection.

In one of my cases a left pus sac had discharged into the rectum and there was present a large, fluctuating mass in the right cul-de-sac. This might have appeared an eminently appropriate case

for attack by the vaginal method, but the procedure had not at that time been introduced, and therefore there was no temptation to deviate from my habit of abdominal section; and indeed it was very fortunate, too, that such was the case, as I found not only a very large tube filled with pus, but also the appendix distended with pus and glued tightly around the cecum, and so large a rent was made in the attempt at separation that a resection of the cecum was necessitated by simple end-to-end anastomosis. Recovery in this case resulted after severe shock, and I am quite certain I should have had a fatal outcome had I first made the attempt to operate by vaginal section, which I should have been compelled to abandon; or, not comprehending the complicated pathology of the case, I would have removed the pus-tube and finished my operation in fancied security, only to suffer the chagrin of witnessing my patient succumb to an early rupture of the rotten appendix. Pryor, the most enthusiastic of the advocates of "vaginal ablation," in replying to the question, "Can you tell through the vagina whether or not the appendix should be removed?" admits that unless such complication can be foreseen his position is untenable, and then replies that "it is time enough to remove the appendix when it produces symptoms requiring it."

It is perfectly clear that on this point Pryor's usually comprehensive grasp of cold facts was relaxed, and sophistry is made to cover a fatally weak point in his argument; for it is incontrovertible that in the case I have just related no sane man would for a moment think of postponing the removal of such an appendix longer than the time necessary for diagnosis, which under the "expectant plan" would have been probably too late. It would have been quite impossible to have made a diagnosis of the conditions found in this case in any other way than by abdominal section; and, although it is barely possible that the pus-tube might have been removed through the vagina, a very grave condition would have been left unrecognized and threatening death, as the gut wall was gangrenous in appearance and would have soon broken down, even if it and also the appendix had escaped injury in tearing away the pus-tube. Certainly, this one case is food for much thought in considering what is best.

In another case a knuckle of gut was caught down against the left broad ligament and formed a horseshoe side to an abscess which began in the left tube and ovary; here again a resection was necessary, and I cannot believe but that in the hands of any operator,

however skilled, nothing but disaster could have promptly attended an attempt to meet the indications by vaginal section.

Since beginning this paper I have encountered a case which emphasizes the dangers of the vaginal method as applied to a large class of cases. Three days ago I operated upon a young woman who had suffered intensely with right pelvic pain ; nothing save tenderness was revealed by digital touch, but when the abdomen was opened the right ovary was found densely adherent to the cecum and had to be dissected away by scissors, and even then the muscular coat of the bowel was bared and I was compelled to close in a small weak point with catgut. I am sure it could not have been accomplished by any other method, nor would it have been good surgery, even if it had been possible (which it was not), to draw the gut down into the vagina (which cannot be considered aseptic) for repair.

It occasionally happens that the bladder is opened into or that a ureter is torn in the vaginal operation. Now, if this were the universally accepted operation, one can scarcely conceive of the vast amount of damage done and suffering entailed in the early efforts of ambitious men before they acquire the skill which is accredited to the expert operator. Incidentally it might be wise to advocate this method in the belief that its very difficulties would discourage a large percentage of slipshod surgeons from persevering in their discreditable work.

An objection to vaginal celiotomy which has not as yet been materially modified is that the wound cannot be antiseptically dressed ; the pelvic floor must be left open and the viscera constantly exposed to the direct contact of whatever infectious material may chance to be present. With care the danger is minimized, but in a considerable degree it is ever present.

In the removal of pus-tubes through the vagina the only mechanical safety is found in " vaginal ablation," as described by Pryor, or in some similar method which likewise involves the removal of the uterus. To my mind this one statement is quite sufficient to condemn the vaginal operation, unless a distinctly lower death-rate can be shown than is charged to the same quality of cases operated by abdominal section.

Conservative surgery is not on the defensive and can never be forced to that false position. Assertions do not make facts, and the physiologists are not likely to concede that because the tubes and ovaries are functionally destroyed, and therefore to be removed,

the uterus may also just as well go. The fact that a woman is better off with a uterus than without one is so universally conceded, and the reasons for it, both moral, mental, and physical, are so well established, that I shall not open this line of evidence in detail.

Another serious objection to vaginal ablation obtains in the subsequent history of the individual so operated. In all cases there is an appreciable shortening of the vagina from the contraction of the cicatrix in the vault. Now, it is very frequently the case that this shortening is very pronounced, and in many cases it amounts practically to an obliteration of the canal so far as practical utility is concerned, and on this account the marriage state becomes intolerable, and the last state of that woman is worse than the first.

In the correction of retrodisplacements the vaginal operation of Dührssen and that of Mackenrodt, Martin's assistant, have certainly distinct merit, but the results are not uniformly ideal. I was in Berlin at the time (July, 1892) Dührssen made his first report of 125 cases, of which he claimed 90 per cent. permanently cured; eight of them had been subsequently confined, and the malposition did not recur. Mackenrodt and Martin about the same time developed a similar operation, differing somewhat in technique, for which they claim superior results. I saw these men do their chosen operations repeatedly, and probably my report was the first detailed description of these operations published in America.[1] I have adopted these methods a number of times, and the results have been fair in most cases and quite satisfactory in some; the greatest objection is that, fixing a comparatively heavy body like the uterus to an insecure anchorage in the vaginal wall, the displacement is very likely to be reproduced, the vagina being dragged downward and backward. I believe the suprapubic operation as modified by Howard Kelly, in which the parietal peritoneum is alone made to support the organ, is distinctly the most reliable to overcome persistent retrodisplacements of the uterus, and so far it seems free from any serious objections.

In order to arrive at a satisfactory conclusion as to the relative merits of the abdominal and the vaginal methods, let us for a moment contrast their claims and defects. In the former we do not get the proper drainage; this is only an occasional misfortune, as the vast majority of cases are distinctly safer without it. In the

[1] Indiana Medical Journal, August, 1892.

vaginal operation we also require drainage in only a *small percentage* of cases, but *must tolerate* it in all cases, certainly increasing the danger in some degree. Therefore the abdominal operation has, in my opinion, the best of the argument on the subject of drainage.

As to hernia, it occurs in such a small percentage of cases as to remove it from serious consideration when contrasting it with an operation so radical as the routine removal of the uterus simply to make the field of operation accessible.

As to conveying sepsis in efforts to reach diseased structures and in dragging them up through the cavity, this is certainly offset in the vaginal operation by entering the pelvic cavity through a non-sterile route—the vagina—and by dragging diseased structures from their points of adhesion in the dark.

It will be remembered that in Pryor's description of vaginal ablation the uterus, together with both ovaries and tubes, is removed; in his report of fifty-four consecutive cases of pyosalpinx he had the rare experience that they were in every instance bilateral. Of course, that fact was in some degree an atonement for the loss of the uterus; but as he must expect frequently to encounter the disease on one side, the other side remaining healthy, he must either admit that there is here a fatally weak point in his system, or else consistently insist on his radical routine and remove a healthy tube, ovary, and uterus in order to render easy the task of removing a single diseased tube and ovary. The only argument which can possibly convince conservative men of the propriety of such procedures is their *constant* repetition without mortality.

The "abdominal scar" will, of course, never be alluded to seriously by an intelligent man; the contracting cicatrix in the vault of the partially extinct vagina does not stare at one quite so boldly; but if the abdominal scar is liable to excite in the mind of the young bridegroom the suspicion that the bride has had her ovaries removed (which objection to the abdominal incision was recently offered), who shall describe his state of mind on discovering that ovaries, uterus, and vagina are gone?

Of course, in bulging pelvic abscesses we must incise the vaginal vault, and occasionally we can more safely handle ruptured tubal pregnancy by the same method. I saved one such case in this manner, making a free incision and packing with gauze, the patient being in collapse at the time. Shock is certainly as frequent and pronounced in vaginal as in abdominal section, especially so when the uterus is to be removed.

In the matter of statistics I have very little to say. I am convinced that if an operator will give every case a chance, not turning the bad ones over to some friend (?), he is bound to have fatalities in his practice, regardless of what method he shall follow; the percentage may be 3, 6, or 8, and he need not blush to admit even the highest rate in some especially difficult lists which fate occasionally brings him. I do not presume to offer any explanation for the flattering statistics offered by Pryor, whom I know personally and respect highly, nor what reason he had for rejecting individuals, nor how it occurred that his friends kept the desperate ones out of his hands; but personally I find it impossible to believe that any man can take one hundred consecutive cases, *without discrimination*, and remove tubes, ovaries, and uterus without a death.

On the whole, I am convinced that if we can devise some practical method of drainage in those cases where it is indicated, we may choose between the vaginal and abdominal methods of *hysterectomy* without having to weigh any very serious questions; but that we are in no way justified in resorting to the so-called "vaginal ablation" as a method of choice in dealing with pelvic disease.

HEMOSTASIS IN THE TUBO-OVARIAN PEDICLE.

By THOMAS BARKER EASTMAN, M.D.,
INDIANAPOLIS.

From the time the immortal Ephraim McDowell made the operation which marked the birth of ovarian surgery to the present the question of hemostasis has been a mooted one. Various methods involving the use of various materials and instruments have been advocated, and yet to-day candor compels the admission that the perfect ligature—a ligature easily rendered aseptic, uniform in tensile strength, reliably and readily absorbable, easily and securely tied—has not been brought forth.

No further proof in support of this proposition need be sought than the increased activity among operators, who, observing and deploring the results of badly acting ligatures, are earnestly endeavoring to find a substitute.

In times but recently passed, operators were so absorbed in perfecting other details of their technique, maintaining sepsis, discussing the advantages and disadvantages of drainage, the merits of the various methods of closing the abdominal wound, and so forth, that they were prone to consider cured a patient who got up from her bed. Now that this technique has reached a stage of comparative perfection, there yet remains upon surgery this stigma brought about by unsatisfactory ligatures. To those operators who feel that in some of the various methods they have ideal hemostasis, this paper will be of little interest. Excluding, as being but modifications of fundamental methods, the various means devised by several prominent operators, as well as those methods which discard all means of hemostasis and depend on so-called blunt dissection, as involving too much risk, hemostasis in the ovarian pedicle is secured in one of three ways: First, by ligation; second, by crushing; third, by desiccation.

The non-absorbable ligature *en masse* may be said to have been the primitive method of hemostasis so far as oöphorectomy is concerned. McDowell dragged the ligatures out of the lower angle of the wound[1] in his first four cases, but in the fifth saw fit to cut off the ligature close to the knot, and had his first death from peritonitis—caused, as likely as not, by an infected ligature and the absence of drainage which the long ligature secured.

Penrose[2] says: "The objections to these ligatures *en masse* are the liability to slip; the difficulty or impossibility in some cases in removing an ovary or a tube; the fact that the broad ligament is puckered up and made more tense than normal, and may for this reason cause subsequent pain and discomfort; an unnecessary amount of tissue is strangulated." He charges the slipping of the ligature and its resultant hemorrhage with being a very common cause of death.

Conceding that the ligature may occasionally slip, it seems not quite fair to point to this as a common cause of death. In support of this opinion, the writer may say that in a thousand or more of the earlier cases in which Dr. Joseph Eastman used the ligature *en masse* no hemorrhage or perceptible hematocele occurred.

If the ligature is properly tied with the so-called surgeon's knot, in such a way that, with the exception of the knot, it is concealed in the fold which its pressure makes in the pedicle, and the pedicle properly shaped, there need be no fear of hemorrhage. With regard to the second objection, the day's literature would lead us to believe that instead of striving to remove every vestige of ovary and tube, surgeons are doing their utmost to leave as much as possible of the adnexa; and what a pity it is that during the days of evisceration furor more of this tissue was not left and all the horrible mental agony incident to an enforced menopause avoided! The two most potent objections to the non-absorbable ligature lie in the fact that, failing to be absorbed or acting badly, it frequently causes more pain than the pathologic conditions for which the operation was made, and leaves exposed a raw surface which affords a particularly convenient seat for adhesions of intestines, omentum, or even the bladder.

Hughes[3] says: "If the ligatures are small and sterile they become encapsulated and rapidly absorbed; if plaited ligatures and large hawsers are used, whether infected or not infected, they are

never absorbed." There is nothing so distressing, so calculated to drive a woman mad, as to undergo a severe operation for pelvic disease, and then, when she believes herself cured, to find herself in a worse condition than before from an irritating ligature. Indeed, the writer had under his observation a case of a prominent songstress who was driven to suicide by just such pain.

When the drainage-tube was in vogue these ligatures frequently worked their way through the tube sinus; but with the growing desuetude of the tube this fortunate issue is not often enjoyed, but the ligature too often reaches the outside through the bladder, the vagina, or through other equally unsatisfactory channels. In one case under my observation twelve ligatures were removed from the bladder, while the thirteenth provided the nucleus of a large calculus. A ligature heavy enough to make absolutely sure against hemorrhage in some of the pedicles is too large to be absorbed and large enough to make trouble.

Recently, I have not gone to the dealer in surgeons' supplies for my silk ligature material, but have been using a No. 5 silk fish line, which possesses great tensile strength in proportion to its diameter, yet is not so fine as to cut, as may occur in tissues long the seat of inflammation. It seems to be readily absorbable. "The fine silk ligatures used in ligating the pedicles become encysted in lymph and remain inoculate. The knotted portion of the ligature remains unchanged, but the loop, if it is a long one, is often dissected apart into its ultimate fibrils by the leucocytes, when the silk is not absorbed, as can always be found."[4] I prefer the use of this ligature in pus-tube cases, as I nearly always see fit to ligate the pedicle in sections. I prefer it in these cases to catgut because of its greater tensile strength and because of the security of the knot.

It has been indeed interesting to observe the increased use of catgut. Operators who four years ago condemned it in the strongest terms as being impossible of sterilization, dirty, unreliable, too quick to absorb, its knots too liable to untie, are to-day lauding it to the skies and using it in many cases altogether. And yet there must not be lost sight of the fact that some very able operators still cling to silk as a ligature, operators who are daily at work, who have unlimited means for sterilization, and whose results any of us might be proud to equal. The failure of catgut in former years

was due principally to the fact that no reliable means of rendering it aseptic had been devised. Numerous methods were brought out, tried in a few cases, the gut found aseptic, only in the next few cases to give rise to the most virulent infection. Today it is not a difficult matter to secure aseptic catgut.

There remain, however, a few objections to this ligature material. But rarely will catgut withstand the tension necessary in drawing a double hitch. Consequently a single hitch is necessary, with its liability to slip before the second knot can be drawn down, although the tendency of this ligature to untie is avoided by making use of several knots. So far as the too rapid absorption of catgut is concerned, chromic catgut has done away with this objection; and yet, as an indication of what might occur in the ovarian pedicle, I have known chromic catgut, after two or three weeks of douching, to come away quite unchanged from a pedicle in vaginal hysterectomy. Again, not infrequently catgut ligatures have been charged with lack of tensile strength when the fault lay, not with the ligature, but with the operator's too long finger nails. Even the best gut ligature may be rent in twain by long, sharp finger nails.

After using catgut prepared by himself, by several different methods, and that prepared by different concerns, the writer has been using with confidence that made by a certain firm whose catgut has proved quite reliable, with the exception that an occasional bottle is found which is shredded, thus depriving it of that strength deemed necessary against hemorrhage.

Taking into consideration the difficulty in rendering catgut aseptic and the skill required in properly trying it, one seems justified in the conclusion that this is not the ligature for the infrequent operator, who, not using enough catgut to warrant much attention in keeping it aseptic, and lacking the constant practice necessary for good ligature-tying, will not find as good results as from properly selected silk.

Within the last few years there has been manifested on the part of not a few operators a determined effort to get away from any and all kinds of ligatures, on the ground that ideal hemostasis could not be secured by this means. The fundamental principle in ligature hemostasis is pressure. The ligature affords continuous pressure, whereas it may be said that the ideal hemostasis should be had by some means exerting for a short time only a pressure sufficiently

powerful to obliterate all vessels. To the disadvantages enumerated against various ligatures it may be added against all of them that ligature-tying in the abdominal cavity requires a great deal of skill. It is a matter of wonder how some beginners, after having seen a few sections made, and with no experience whatever in ligature-tying, are able to extirpate a pus-tube from its bed without having a fatal hemorrhage. To dispense with the other objections to ligatures, rather than with the difficulty of tying, Doyen, Tuffier, and Thumin have devised instruments which depend for their hemostatic effect upon the exertion of enormous pressure upon the vessels.

According to Bissell,[5] "the vessels at the point of greatest pressure have their inner tunics ruptured, which incurvate, resulting in the formation of a clot. These facts are essential to the understanding of the subject—viz., a systematic application to tissue *en masse* of pressure sufficiently great to immediately and permanently check bleeding from all vessels within compressed area."

The method is new, and while it numbers among its advocates some very able men—as, for instance, Landau, who reports twenty-seven cases without a death—it is yet too early to give to these instruments a permanent place in our armamentarium.

However, those who have used it express themselves in terms of strong approval. Stone[6] states that the angiotribe as a compressor is nearly perfect in its actions. However, he says "that in all necrotic tissues or in myomatous or omental structures its work has not been altogether satisfactory, and further study of its action is necessary." Bissell[7] calls attention to the fact that "there must always be sufficient structures between the organ or tumor and the remaining tissue to be completely surrounded and compressed. Weak, degenerated, or friable tissue, such as we find in malignant growths, breaks down easily under pressure and bleeds."

While many of the users of the angiotribe indorse it as an excellent instrument, this opinion is not universal; as, for instance, Grandin,[8] in discussing Cleveland's paper, says: "It seems to me, however, that it is an innovation which departs from well-established laws of surgery. I see no use for this new instrument. I am upholding the surgical practice of tying a vessel against the practice of burning or crushing a mass of tissue and leaving it in the abdomen to necrose. The principle on which this instrument

works is fallacious, and it comes to us from the prolific ground—Europe—from which so many false gods come to us."

Ratchinsky[9] says: "The angiotribe cannot always be fairly applied, and in some instances retention forceps and ligatures must still be required." He further concludes that angiotripsy with Tuffier's instrument is thoroughly practicable, but that the hemostasis is not always permanent. While the principle is correct, the failure to obliterate large vessels exposes the patient to unnecessary risk.

Years ago Keith, appreciating the disadvantages of the ligature, devised his clamp-and-cautery method in dealing with the pedicle, and to this procedure is due a great portion of the credit for this then unheard-of and unequalled record. This method was not followed by other operators to any considerable extent, probably on account of the manual dexterity required in properly carrying it out, and because those who attempted it did not realize that its results did not depend upon cauterization, but upon desiccation, and that a red-hot cautery will cut an artery and leave its lumen as little occluded as if done by a sharp razor.

Having in view the unsatisfactory results of ligatures and disadvantages of the cautery, Skene set out to perfect an instrument for hemostasis and brought forth his electro-hemostatic forceps. This instrument has been in the operating-room for such a length of time that an opinion may be formed as to its merits. It is too well known to require any detailed description, except to emphasize the fact that its efficiency is had, not through charring the pedicle, but rather by desiccating or cooking it. The larger forceps is so constructed as to give a considerable degree of pressure, and thus in a measure combines both the good points of the angiotribe with the advantages of desiccation. I have used this instrument a considerable number of times, and while it will not prove satisfactory in all cases, I believe that if used with judgment it cannot fail to be a valuable addition to any surgeon's armamentarium. I must confess that it was with some misgiving that I dropped my first pedicle, a rather large one, after having used this forceps, but a few cases gave me such assurance that I now dismiss them with as little fear as when a ligature is used. It absolutely does away with sloughing tissue beyond ligatures. Used in pus-tube cases by the application of another traction forceps, it is possible to prevent the

escape of pus, as sometimes occurs when the distal end is severed. It should be applied in the long axis of the superior border of the broad ligament, and on removal leaves but a small, thin membrane of cooked tissue. Here at once becomes apparent a most desirable advantage of the instrument. It does not put the broad ligament on a severe strain, thus giving rise to great pain immediately after the operation and sometimes for years subsequently. There is no considerable mass of tissue held in the tight grasp of a ligature pressing on a sensitive nerve, for the nerves are devitalized; nor is there danger of a Fallopian tube becoming patent, with its resultant secondary inflammation of stump and formation of adhesions. This is perhaps the only method by which these results can be avoided. Patients are peculiarly free from pain after these operations. On one occasion, where, in a pus-tube case, the tissues were rendered friable by inflammation and my ligature seemed to cut, I was enabled to use this forceps with most agreeable results. Its greatest disadvantage seems to lie in the fact that it cannot be used away from electric currents, as storage batteries are too cumbersome for transportation. Indeed, it seems to be an ideal instrument.

To summarize: 1. The silk ligature is still used by many operators of ability. 2. A silk ligature of proper size has most advantages over catgut. 3. The silk ligature is the ligature for the beginner and infrequent operator. 4. Given an aseptic, strong catgut ligature, it is perhaps the best for the skilful ligature tyer. 5. The angiotribe represents a step in the right direction, but is not yet perfected. 6. The electro-hemostat lacks but little of being an ideal hemostatic, and should come into general use in the treatment of the tubo-ovarian pedicle.

In conclusion, the writer is of the opinion that he will have best success with the ovarian pedicle who confines himself not blindly to any one method, but rather uses a wise discrimination between the various methods now at hand.

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THE NOSE A FACTOR IN POST-OPERATIVE DISEASE.

Report of a Celiotomy followed by Malignant Septicemia of Nasal Origin; Further Observations; Remarks.

By H. O. PANTZER, M.D.,
INDIANAPOLIS.

Mrs. McC. was referred to me by Dr. Will F. Green, of Shelbyville, in June, 1896, for treatment of a subinvolution and retroflexion of the uterus. Patient was a woman of strong build, aged thirty-two years, mother of one child. She suffered sacral and occipital pains, dysmenorrhea, and great physical and psychical depression. No conception since the childbirth eight years ago.

My examination of the pelvic organs confirmed the diagnosis of the family physician. The womb was found impacted, though reducible. There was no evidence of inflammatory complication. The ovaries were prolapsed, tender, and showed small cystic enlargement. I decided to curette the womb; then, by celiotomy, to incise the small cysts of the ovaries and ventrofixate the uterus. The patient was under my observation three days before the operation. The temperature during this time varied between 98° and 100.2°, and the pulse between 82 and 100. These deviations from the normal were not explained after a searching local and general examination. In the absence of a better explanation they were ascribed to psychical excitement.

Operation at my sanitarium on June 27, 1896, under chloroform. The diagnosis was confirmed. No evidence of inflammation in the pelvis, obselete or active, was found. An afebrile, uneventful recovery was anticipated.

The course of the case subsequent to the operation was interesting from the start. The temperature at midnight of the day of the

operation was 101.2°, pulse 98. Patient restless and complaining of drawing sacral pains. Abdomen flat, not tender. Bilious vomiting and retching. There was a profuse nasal and pharyngeal secretion, but no more than is frequently present after narcosis ; it was attended with hoarseness and some cough. Feeding by mouth was unsatisfactory, and rectal alimentation was kept up from the beginning. The temperature continued febrile, being 103° much of the time, and reaching 104° on the sixth and seventh days. The pulse commonly varied between 88 and 100 and was fair ; latterly it rose in spurts to 140 and more beats, and at such times showed irregularity and bad quality. Internal antiseptics, antipyretics, and stimulants were employed, seemingly without effect. The skin soon became dry and showed a septic, yellow color. Sensorium was free. At times of the higher fever and pulse the patient developed euphoria. The restlessness of the patient and the lack of solicitude about herself were striking features of the condition. The rectal discharges marked no departure from the normal. The abdominal wound and the pelvic viscera were repeatedly examined, without finding anything wrong. The abdomen remained free from tenderness. Bloating was transiently present in moderate degree ; it yielded readily to the rectal tube.

The unsuccessful search for the cause of the septicemia was distracting. I felt yet I must associate it with the operation. The uterine cavity was swabbed with 25 per cent. carbolic acid, though there was no condition present to indicate such procedure. The sutures fixing the uterus to the abdominal wall were released the third day in the faint hope that this might relieve nervous tension. On the night of the fourth day the abdomen was opened under chloroform, but nothing abnormal was found. The uterus was again ventrofixated. Subsequently the sutures holding the uterus were again removed. Meanwhile the patient's general condition had grown critical. She was weak and helpless to the extreme, and the end seemed not far. The secretion from the nose and throat continued. It was observed that it had increased since the second narcosis, and that it had grown somewhat fibrous and purulent in character. Hoarseness and aphonia had gradually developed.

At this stage the case recalled to my mind a short and wilting septicemia which I had suffered when I had had a spur removed from my nose some time previously. At the time, when I mentioned my experience to the nasal operator, he disclaimed septicemia of such origin. To me, however, the nasal operation and the fol-

lowing septicemia seemed intimately associated. I considered the possibility of a nasal origin of the septicemia in my patient. The nose and throat were vigorously cleansed with peroxide of hydrogen. Before this gargles of borated water and bicarbonate of sodium solution had been used. The use of the peroxide of hydrogen was followed by early marked improvement. The temperature was taken hourly for two days. At this time—the seventh day after the operation—it registered 104°. After the second nasal douching it fell to 102°, and soon it fell to normal. It rose a few times, in spurts only, lasting an hour or two. After the tenth day the temperature continued normal. A bacteriologic examination of the nasal and laryngeal secretion unfortunately was not made.

Further inquiry into the history of this patient ascertained that she had had several attacks of acute nasal and laryngeal catarrh. Aphonia, stridulous cough, great lassitude and depression, followed by slow recovery, were the characteristic features of these attacks. The family physician had designated the attacks as croup. The attack observed by me undoubtedly was of like character, though possibly in degree was more severe than any former attack.

After leaving me the patient had her nose treated by a nasal specialist. Following an operation on the nose the patient suffered another severe though aborted attack of septicemia, during which she lost her voice. Since then the nasal disease has been cured and no more attacks have occurred.

It is remarkable in this connection that text-books on the nose have nothing to say on febrile temperatures, sepsis, infection, etc., either as pertaining to the sequelæ of nasal operations or to the ordinary non-traumatic diseases. I say this after perusal of the last edition of each of F. H. Bosworth and of Fletcher Ingals. Rhinologists to whom I have addressed myself tell me that other special works are equally silent on these matters. From *a priori* reasoning there appears no ground why septicemia should not attend upon nasal disease and nasal operations. That the grave septicemia in my case was of nasal origin seems to me incontrovertible ; also, that the neglect of the nasal treatment would have entailed the death of the patient.

The nasal cavity naturally, and even more by the deformities its diseases develop, has nooks and corners, crevices and ledges, which arrest the microbes of the inhaled air and retain the nasal secretions. Here are given *ne plus ultra* conditions for bacterial

propagation. Toxic absorption and infection must and do occur. The malaise, mental dulness, glandular stagnation, sallow septic color, so invariably present in almost every case of nasal catarrh, are rationally accounted for by the assumption of septicemia.

The lesson of my case for me has been to look for nasal complications in all cases. In the three years that have passed I have found noteworthy nasal complications in about 30 per cent. of all cases. Persistent vomiting after operation can be owing to it. The nasal disease acts in a twofold way to produce this—namely, mechanically, by causing hawking and spitting and ultimately disquieting the stomach ; and chemically, through the agency of the nasal toxins. Headache, restlessness when awake or asleep, insomnia, anorexia and indigestion, coated tongue, were frequently relieved or mitigated by nasal treatment. Slight elevations of temperature, otherwise unaccountable, were at times controlled by nasal treatment. I have observed only one other case of grave nasal septicemia such as the case reported. I must accept that such cases are rare. How much my treatment forestalled grave symptoms in other cases I do not know.

The many times that I found nasal disease complicating a recovery from an operation or complicating other diseases have led me to ask myself what conditions there were that especially favored nasal pathogeny in my cases. I would enumerate as such conditions: First, the acute transition, commonly, from an active life to passivity, and from an intermittent erect and prone position to one of continuous recumbency ; and, second, the devitalizing effects of the chloroform and the operation.

The acute change from an active life to one of passivity, while commonly easily met by the system, yet entails perceptible difficulties in those weakened by disease. The powers of accommodation, anyway, are variable in different individuals. This, though a minor factor, deserves mentioning, because it is superadded to the derangement of circulation resulting from prolonged recumbency. Continuous recumbency involves great changes in the circulation, or, more specifically, changes in the hydrostatic conditions of the body. We note that the subject of nasal disease awakens from dreamful and disquieted sleep with a dull mind, a foul tongue and anorexia, and a lethargic body. These, as the morning passes, are steadily dispersed. During recumbency there is increased flow of blood to the head, which, owing to the nasal occlusion, has deficient return flow. In turn, this hyperemia entails an interference

with nasal and pharyngeal glandular function. The hyperemia and glandular stagnation are measurably relieved by the upright position. During continuous recumbency, however, the consequent morbid changes are increased in degree and likely involve bacterial propagation. This latter is probably minimized when the recumbency is of the ordinary short duration. The subject of nasal catarrh under ordinary conditions holds the prone position only one-third of the day. Yet this time suffices to produce evidence of congestion and septicemia. This being true, the deleterious influence of prolonged recumbency must be expected to be proportionately greater. The natural powers undoubtedly in many cases mitigate and overcome these evils or establish a tolerance of them. But at all times such conditions have pathologic significance, varying from a slight transient malaise to the malignant septicemia which was manifested in my case.

Secondly, there is the local and general effect of a surgical narcosis to increase the liability to nasal developments. It is generally accepted that chloroform and ether devitalize the tissue cells with which they come in contact. Then, too, the general circulation and the nervous system suffer disturbances and depression. These effects favor saprophytic and pathogenic bacterial processes within the nose and throat, and, indeed, the body. In the case related the narcosis was decidedly helpful in promoting bacterial activity. It may be assumed that the special bacterium involved was domiciled in the nose and its vicinal cavities. There was a distinct increase of the nasal and throat symptoms immediately following each narcosis.

The physical conditions pertaining to a recumbency the result of internal disease differ only in degree from those entailed by surgical procedures. To this extent my observation and deductions hold good and apply to internal cases as well. Indeed, in looking back upon my experience in general practice, I discover quite a number of instances where the nose might justly be accused of complicity in the etiology and the gravity of the disease.

In conclusion, it is difficult to say to what extent it is justifiable to suspect nasal septicemia as a cause of death in instances where the post-mortem finding has failed to reveal adequate cause of death. It is certain that the nose has heretofore been disregarded, except, perhaps, in those cases where diphtheria or erysipelas was recognized before death occurred. When once the nose is closely scrutinized for evidence of disease, it may be it will be connected with otherwise unexplained deaths. Had my case died without

there having been previously detected the nasal disease, the case would have been added to the humiliating list of post-mortems where the finding falls short of assigning an adequate cause of death. Then, probably, we would have been driven to say that the septic character of the infection was such that the smallest quantity of its toxin had extinguished life even before a demonstrable lesion could develop!

Recently, the following case came under my observation, which may fitly be added here. A young man of fine physique and apparently resplendent health was suddenly stricken with appendicitis. The operation, undertaken for its relief within thirty hours after the first appearance of acute symptoms, was without complication, and an uneventful recovery seemed promised. The wound and the abdomen at no time showed evidence of trouble. Thirty-six hours later symptoms of an impending meningitis developed, and death from this cause supervened in twenty-four hours more. The patient, it was ascertained, had suffered daily recurring nasal occlusion with "full" head and painful eyes in the preceding six or eight weeks. Relief had come each day with a nasal hemorrhage. Slight hemorrhage and occlusion of the nose were observed on the second day after the operation, but were not promptly acted upon. The nasal process had evidently existed a long time in dangerous proximity to the cerebral meninges. It may be accepted that the effect of the narcosis and the prolonged recumbency sufficed to overcome the barriers which had heretofore successfully withstood the impending cerebral invasion.

POST-OPERATIVE ACUTE TOXIC HYPEREMIA AND INFLAMMATION OF THE KIDNEYS.

By F. F. SIMPSON, M.D.,
PITTSBURG.

THE varieties of nephritis and their etiology, pathology, symptomatology, etc., are matters of common record and are easy of access in current and classical literature. Reference will be made to those forms only, of acute toxic hyperemia of the kidneys and even nephritis, which occur after surgical operations. Regarding this condition Hall[1] says: "Except septic infection, this is the gravest complication following a section with which we have to contend." Delafield[2] states that "it is not so well known that surgical operations on any part of the body are occasionally followed by suppression of urine, congestion of the kidneys, and death. We do not know that death in all these cases is caused by the congestion of the kidneys, but there can be no doubt that congestion of the kidneys is produced in this way." That the condition may exist with or without serious symptoms has been repeatedly demonstrated by many careful observers the world over, who habitually cause a systematic study of the urinary secretion and constituents to be made prior and subsequent to anesthetization and operation. I am convinced that it is of serious moment, as is every complication, however infrequent it may be, that places the life of a patient in jeopardy and rarely determines a fatal issue. Its causes and significance, therefore, are matters for most pertinent inquiry.

Etiology. The frequency with which abnormal urinary constituents have been observed in post-operative cases with previously healthy kidneys, points strongly to a causal relation between the operation and preparation therefor on the one hand, and the kidney complication on the other. By recalling the generally recognized causes of nephritis it will be found that the most common are

almost habitually present, viz., exposure to cold and dampness, with chilling, not alone of the surface, but of the lungs as well; and the absorption, circulation, and elimination of toxic agents—*i. e.*, anesthetics, ptomaïnes, leucomaïnes, and other toxins. The effects of these causative agents will be considered *seriatim*. Predisposing conditions are probably more frequent than in a like number of healthy individuals.

In a carefully prepared paper read before this Association at its meeting held in Pittsburg in September, 1898, Dr. Rufus B. Hall,[1] of Cincinnati, discussed the results of exposure of the patient to dampness and draughts during and after operation. His remarks were thoroughly in accord with the views generally entertained with regard to the etiology of nephritis, and served to emphasize the importance of a grave error in surgical technique. To appreciate in a measure the effects of such exposure, varying in duration from a few minutes to more than an hour, it is only necessary for the surgeon to stand for a moment in a draught before removing his damp operating suit.

With reference to the irritating effects of ether and chloroform, and their products of elimination, upon diseased kidneys and other tissues with which they come in contact, opinion is positive; regarding the degree and results of that irritation it is divided. That the tendency to irritation is not always in evidence is well known. That appropriate operation often relieves a hyperemia, and at times a true nephritis, is beyond question. The reason seems to be the removal of a cause more potent and persistent in its pernicious effects than the anesthetic.

Opinions of eminent men regarding the effects of ether and chloroform upon healthy kidneys are by no means uniform. In many instances they are widely divergent. Whether this difference results from the fact that some opinions are based upon scientific experiments with all conditions under direct control of the observer, while other opinions rest upon clinical evidence with many perturbing factors, is not clear.

Weir[3] concluded, from a study of 305 cases, "that etherization in the vast majority of cases in normal kidneys, and even in abnormal kidneys, brings about no detrimental effects; that when the evidences of abnormality present themselves they are transitory in character and not productive of harm; that elevation of tem-

perature, which I had before thought would aggravate the work of the kidney and bring about, in conjunction with an ether narcosis, abnormal excretions, does not appear to exercise any positive influence on this point." Yet the tabulated statement from which these conclusions are drawn shows "normal before, afterward abnormal, 31," and he says regarding them, "In nearly all of the cases marked 'abnormal afterward,' the albumin or casts disappeared or decreased within a few days after the etherization." He thus admits that an abnormal condition of the kidneys frequently exists, but has evidently been fortunate enough to see few if any unfavorable results therefrom.

He gives the following conclusions drawn by the authors named. Fenter, of Berne: 1. "That ether has no perceptible effect upon the healthy kidneys of animals, which, moreover, are more susceptible than mankind to its influences. 2. That it is not dangerous in persons whose kidneys are slightly diseased. 3. That subsequent disturbances in the circulation of the kidneys, when met with, are very transitory and rapidly disappear." "Butler, in 500 cases of ether narcosis, found only once albuminuria where previously none had existed. Körte, in 600 cases that he collected, found only six instances where albumin presented itself where before the etherization none had been found, and he states very decidedly that ether does not aggravate a damaged kidney, but that chloroform does. . . . Garré[1] says very emphatically that he does not believe an attack of nephritis is at all likely to occur from etherization. Wunderlich concludes that in an already existing case albuminuria is frequently augmented by an ether narcosis; that this is twice as common in chloroform narcosis." The possibility of irritation is thus conceded, yet he adds, finally, that "ether nephritis may be excluded from surgery." Keen and Da Costa[4] state that "Trieber and Roux did not find an instance of albuminuria in several hundred cases." The same authors give Selbach's[5] conclusion that "even prolonged etherization never produces nephritis."

On the other hand, Thompson and Kemp[6] conclude from experiments on animals that ether has a specific effect upon the kidneys, lowering their blood-pressure out of proportion to that of the carotid, and that the quantity of urine decreases as narcosis becomes more pronounced; that albumin appears early and progresses with

narcosis; that mixtures containing ether have an analogous but less marked effect; that chloroform narcosis causes albumin in less amount, but the circulatory discrepancy is not noticed. The very careful experiments made upon dogs by G. B. Wood[7] show that immediately and some time after ether narcosis the kidneys appear congested, and that microscopic examinations, confirmed by Guitéras, reveal a pronounced cloudy swelling of renal epithelium. They further show that repetition of anesthesia during several consecutive days gives rise to a true desquamative nephritis.

The following evidence, though in accord with these experiments, is subject to the fallacy that other factors than the anesthetic may have been present, and their influence, however slight it may be, must not be disregarded. Of 70 patients taking chloroform, Patein[8] found that 12 per cent. had pre-existing albumin, 15 per cent. albumin after anesthesia but before operation, 73 per cent. after operation. Blake[9] says that "so-called ether nephritis was first described by American writers, and has never been found to any considerable degree except by Americans." He further says: "The writer has examined 50 cases, before and after ether (urine filtered and nitric acid test applied; the presence of a whitish precipitate or cloud at junction of acid and urine, visible against a dark background, was considered to be evidence of an appreciable and abnormal amount of albumin)." Of these 50 cases, 33 did not contain albumin before ether. In 25 cases that contained albumin after ether and none before, "2 presented a moderate amount (large trace), 4 a slight amount (trace), and 19 an extremely slight amount (slightest possible trace). No thorough examination was undertaken in regard to the duration of albuminuria. From occasionally detached observations, it seemed, as would naturally be expected, to be of very short duration. Examination for casts was not made." Deaver[10] concludes, after observations upon many cases and a systematic study of urine for ten days following operation upon 62 patients whose urine was previously normal, ether being the anesthetic used, that "consideration of these results leads us to the conclusion that ether has a very considerable irritating effect on the kidneys." He expresses the same view regarding chloroform. He states that Israel found abnormal urinary constituents in 33 out of 100 cases of chloroform narcosis. Hall[11] records 110 sections under ether, all kidneys normal previously;

33 had a trace of albumin the first day; 10 had partial or complete suppression from one to four days; 2 died in coma. Four hundred and sixty-five sections under chloroform, with previously healthy kidneys, showed 58 cases of albuminuria. No deaths from nephritis in this number. (He also gives cases with pre-existing nephritis, but they are omitted because irrelevant.) He says: "We found the irritating effect of the ether so frequent that we abandoned it, and would not willingly go back to its use again." Carl Beck[11] found that the urine of 27 of 300 etherized patients with previously healthy kidneys contained albumin thereafter. Eisendrath[12] believes that both ether and chloroform are distinctly irritating to the healthy kidney; that the frequency with which renal elements appear in the urine after narcosis proves this fact; that abnormal constituents disappear within twenty-four hours; that chloroform has a much more pronounced effect than ether. Kelly[13] expresses the opinion that "acute nephritis rarely follows an operation except where there has been pre-existing disease."

The following brief summary is made from the records of the last thousand patients subjected to major operations in the Gynecological Department of Mercy Hospital. For the privilege of observing these cases during their convalescence I am most grateful to Dr. Werder, in whose service they occurred.

For many years it has been a custom to have repeated careful urinary analyses made prior to operation. Where casts or albumin were found and early operation was imperative, chloroform was given. Where operation could be postponed, attention was given to the kidneys and operation done when they became more nearly normal.

Some time ago there occurred within a few weeks two or three cases of marked hyperemia of the kidneys with pronounced symptoms. Since that time it has been customary to have urinary analyses made the day following operation, and as often thereafter as indicated, if evidence of irritation is found. The urine of the first 261 reported cases was not systematically examined after operation. Only rare instances where symptoms suggested trouble were thus studied. Probably some of the earlier cases with symptoms were overlooked. Only 6 cases of this number, which were previously normal, are recorded as having urinary and systemic symptoms of kidney insufficiency; 5 responded promptly to treatment;

1 died. In the remaining 739 cases the average daily secretion for the first three days was about fourteen ounces. Urinary analyses were made before and after operation. Neither albumin nor casts were found before operation in 642 of this number. After operation 175 of them contained casts. A few less contained albumin also. About 135 of these cases presented no constitutional symptoms whatever. The quantity of urine was not below the post-operative average; albumin was in very small quantity, and a few hyaline and granular casts soon disappeared. Thirty others, the majority of these being operated upon for inflammatory conditions of the pelvic organs, presented pronounced evidences of systemic trouble. In the remaining 10 the symptoms were alarming. One died. In 2 fibroid cases and in a few others casts persisted for some weeks, without albumin or other symptoms, however.

The generally observed sudden marked diminution of urinary secretion occurring coincidently with anesthesia, and followed soon by the appearance of albumin and casts in the urine, without symptoms suggestive of other poisons, when considered in connection with the experiments upon animals previously referred to, leaves room for but little doubt that the anesthetic is capable of causing at least a hyperemia of the kidneys. When these abnormal constituents persist for weeks without special symptoms, it looks as though an actual nephritis of low grade had been developed. If pronounced symptoms supervene at once, and if the symptoms persist, the possibility that the poisons which cause the symptoms may be irritating to the kidneys and actually intensify what was originally merely a hyperemia is not unlikely. It is this possibility and often this condition which, I think, renders the irritating effects of anesthetics a matter of no little consequence.

To attach importance to the last-named causative factor—*i. e.*, the absorption, circulation, and elimination of ptomaïnes, lucomaïnes, and other toxic agents—it will be necessary to show their existence, their irritating effects, that they are normally eliminated by the kidneys, and that poorly acting kidneys cause their retention with cumulative effects due to these specific poisons. I believe that such poisons are often present far more often, in moderate degree, than we care to think or admit, the symptoms they cause usually being slight and considered as essential features of the healing process, just as "laudable pus" once was. An actual demon-

stration of their presence would be difficult. In its absence conclusions based upon that supposition must be subject to criticism and doubt.

That abundant opportunity for the generation of such poisons exists is shown by the following considerations: Familiar experiments by the best authorities have demonstrated beyond cavil the normal existence of organisms in the skin of surgeon and patient and that it is impossible to render the hands sterile. It is a matter of common observation that a good culture medium often awaits the introduction of more or less intensely virulent organisms. With or without culture medium, the healthy peritoneum or a fresh wound elsewhere is capable of destroying and disposing of a considerable number of pathogenic, even pyogenic, bacteria, as has been clearly shown by the experiments of Muscatello, Wegner, Grawitz, Pawlowski, Reichel,[14] Robb and Ghrisky,[15] and many others. An occasional stitch abscess or infected pedicle shows that this is not always effected without detriment. Fortunately an intelligent and conscientious use of rubber gloves, in conjunction with the strict observance of an aseptic technique, has materially lessened the number and frequency of post-operative sequelæ. But perfection has not yet been reached, and until that time comes we must expect occasional evidences of infection.

The frequency with which nephritic complications occur in the course of the acute infectious fevers is evidence of the irritating effects of their toxins. "Scarlet fever is said to be the mother of acute nephritis" (Whittaker[17]). Regarding the etiology of acute parenchymatous nephritis, Delafield[2] says: "Unquestionably, all the infectious diseases are often complicated with inflammation of different parts of the body. The probable causes of these are the chemical poisons produced by the growth of the pathogenic bacteria belonging to each disease."

The effects of the specific poisons are so well known as to require no further comment, save that in a limited observation, embracing hardly more than 20 cases of diffuse septic peritonitis from all causes in which urinary analyses were made, I do not recall one in which albumin and casts were not found. This statement has not been verified by reference to records, but is certainly not far from the truth. (It is needless to say that reference is not made to the cases of pelvic and other varieties of localized peritonitis that are

of frequent occurrence and which often have associated evidences of kidney irritation.)

That these poisons are normally eliminated by the kidneys the following quotation from Vaughn and Novy[16] will show: "It is now a well-established fact that the urine of certain infectious diseases, as cholera (Bouchard), septicemia (Feltz), etc., is far more poisonous than normal urine. That the poisons, basic or otherwise, which are generated within the body by the activity of bacteria can be excreted in the urine, is seen in the fact that immunity to the action of bacillus pyocyaneus has been conferred on animals by previous injections of urine taken from animals inoculated with that bacillus (Bouchard) or with filtered cultures of the same (Charrin and Ruffer).

"Furthermore, the excretion of the tetanus and diphtheria poisons by the urine has been shown to take place. Thus, Brunner demonstrated the tetanus poison in the urine of experimental animals, but failed with the urine of the disease in man. Bruschettini, however, with the urine of a tetanus patient, produced tetanic symptoms in mice. In the urine from diphtheria patients Roux and Yersin demonstrated the presence of the diphtheritic poison by inducing paralysis in animals."

It is generally conceded that constitutional symptoms due to local foci of infection result from the elaboration, absorption, and circulation of specific poisons.[16] The evidences at my command of the cumulative effects of toxins are entirely of a clinical nature, and will be discussed when the symptoms of this condition are being considered.

Symptoms. The symptoms of this affection of the kidneys are urinary and systemic.

The urinary secretion is markedly diminished after every major operation requiring an anesthetic, and especially so after operations involving the peritoneum. The average amount in 150 sections as given by Clark[13] was about 500 cubic centimeters a day for the first three days, during which it varied but little. The normal quantity was reached in ten days. This fairly represents the estimates of most observers. The specific gravity is correspondingly high. In the vast majority of these cases nothing but the merest trace of albumin and a very few hyaline and pale granular casts are observed, casts being found more often than albumin. In the

mildest type of this condition the abnormal urinary constituents appear within the first eighteen hours, are more pronounced for from twenty-four to thirty-six hours, and disappear within thirty-six to seventy-two hours. No other symptoms are noted. In this class it is likely that merely a hyperemia exists.

The symptoms appear to be more pronounced in about 16 per cent. of the cases in which nephritic disturbance is found (about 3 per cent. of the total number of post-operative cases observed by the writer).

In these the subnormal urinary secretion continues; the abnormal constituents are in greater abundance and persist for some days—in a few instances some weeks—longer. The vomiting, which usually subsides within a few hours, returns at the end of twenty-four hours and varies with the impairment of function. It may be spontaneous or induced. Nausea is much more marked than in the primary vomiting. To this return of vomiting I have come to attach much significance, and believe that it is far more often a result of sluggish kidney action than is generally conceded. I think its persistence or return should invariably suggest and demand an immediate and careful urinary analysis.

In a few of the cases observed the symptoms were serious and quite unlike those found in the same grade of irritation in non-operative cases. They were strongly suggestive of beginning peritonitis, but I do not believe peritonitis existed. Vomiting persisted or returned. The pulse rapidly grew more frequent and less strong, reaching 120 to 140. The temperature was variable, usually elevated, sometimes reaching 101° to 102°, and not falling promptly to normal as after the usual section. The abdomen was more or less distended, but *not* tender. Peristalsis was sluggish. The urinary symptoms, as would be expected, soon became more pronounced than in the foregoing cases. The quantity at first was less than in health, but for the first few hours the normal amount after section did not increase, but progressively decreased. High color and specific gravity were noted. Albumin was present in most cases, absent in a few. Casts, at first only hyaline, within a few hours became numerous and all varieties were found. With this increase the symptoms grew more marked. In the favorable cases appropriate remedies caused the skin, bowels, and kidneys to act more freely, and the symptoms promptly subsided. In a very

few cases—two in one thousand—the nephritis seems to have had a definite determining effect in causing death. The foregoing classification does not embrace cases in which nephritis merely complicates a diffuse septic peritonitis, to which latter condition it is in every way subordinate.

It will be noted from the symptoms detailed that edema was not once observed; that there was never a slow, full, bounding pulse; and that in no case were convulsions or coma present. Yet most writers on this subject attach too little significance to post-operative kidney lesions, presumably because of the absence of these classical symptoms of uremia. Thus Kelly[13] says: "Although acute congestion of the kidneys or acute nephritis are often assigned as the cause of death after surgical operations, I am unable to find a single record of such a case either in my clinical histories or autopsy records. In many instances a temporary increase in the amount of albumin and in the number of hyaline and granular casts, which have been present before operation, is noted, but in no instance has the patient shown signs of *uremia.*" He further asserts that "following all operations, especially the graver abdominal ones, there is a marked diminution in the amount of urine in twenty-four hours. It is, however, of little import and need occasion no alarm, so long as it does not persist *and there are no symptoms of uremia*" [italics mine].

I think it will be generally conceded that the symptoms of nephritis are due to an impairment of the function of these important excretory organs and the consequent retention within the system of poisons that should be eliminated by them. The symptoms, then, must of necessity vary with the kind of poison retained. *In the affection under consideration the retained toxic agents do not cause uremia.* They cause a definite train of symptoms when absorbed in excessive quantities, and the same symptoms are seen when they are *retained* in excessive quantities. Observations upon many cases in which it was impossible to control oozing, and a pelvovaginal gauze drain was inserted to favor discharge without absorption of poisonous fluids, tend to confirm the view that symptoms of post-operative kidney lesions are due to sapremia with faulty action of the kidneys and an imperfect elimination of toxins.

In several of the cases alluded to the gauze did not drain well and

the symptoms were identical with those observed in the few cases with serious symptoms from post-operative nephritis. In the latter cases improvement followed immediately upon increased urinary secretion, just as it did when the gauze was removed and the cavity thoroughly washed in the retention cases. In one class the supply was cut off and absorption stopped; in the other an excessive quantity of poison which had been retained in the system was speedily eliminated. In either case failure to remove the cause of trouble would have placed the patient's life in serious danger.

The opinion expressed above as to the cause of the symptoms of nephritis is not without precedent. Whittaker[17] says of the nephritis occurring in the course of scarlet fever: "The typical nephritis presents, as a rule, a picture very different from that of ordinary albuminuria. It distinguishes itself by the gravity of the nervous symptoms, by the extent of the dropsies, as well as by the marked changes—presence of blood, reduction in quantity, even to anuria, etc.—in the urine. It distinguishes itself further by the fact that even the gravest symptoms do not preclude recovery." This strongly suggests that the symptoms are not due so much to the local lesions as to the quality and quantity of the poison retained. Forschheimer[18] states that "the peculiarity of the scarlatinal edema is that it rapidly becomes anasarca. Leichtenstern believes that the irritation of the scarlatinal poison takes place in the lymphatics. Cohnheim believes that it is due, in common with other forms of dropsy of the skin, to changes in the bloodvessels. The fact remains, as stated above, that this condition is almost characteristic of scarlatina." Thompson[17] asserts regarding diphtheritic nephritis that "it is exceptional for it to occasion uremia or edema."

Though the analogy between post-operative nephritis and that occurring in the course of the acute infectious fevers is striking in many respects, some important differences exist—viz.: 1. In the post-operative variety the system is well cleansed of the ordinary effete substances by the preliminary treatment, and hence the absence of the usual symptoms due to them. 2. The heart and vessels have been depressed, and hence the absence of high tension pulse, etc. 3. In the post-operative variety the inflammation is not *caused* by the specific poison, but by the anesthetic, etc., and is intensified by toxins, thus rendering it possible for a smaller quantity of these agents to do more harm than in the specific fevers.

4. The specific poisons produce their own effects and cause their own series of symptoms in the respective conditions.

Treatment. The treatment is prophylactic and curative.

The preliminary investigation of a case should always satisfy the surgeon that the kidneys are acting freely. Should any abnormality be detected in the quantity or composition of the urine, its causes should be determined and appropriate remedies used. It is quite as essential to have well-acting kidneys as well-acting bowels.

Exposure of the relaxed surface of the body at a time when the patient is wholly unable to resist its deleterious effects must be avoided. An extravagant and ill-directed use of water is to be condemned, for it saturates the coverings of the patient, and evaporation causes chilling. It is the duty of the surgeon to scrutinize every detail of preparation.

The anesthetic should be administered by one thoroughly conversant with its *remote* as well as its immediate effects. His attention should be given to that alone, and the least possible quantity should be used.

The preparation for every operative procedure should be made with as much care as is taken by the bacteriologist in the isolation of organisms; for in the one case a life is at stake, in the other an isolated fact is to be settled. The proper use of sterile rubber gloves in conjunction with an otherwise aseptic technique is of great value.

The curative treatment is the same as for acute hyperemia or inflammation of the kidneys due to other causes, and is too well known to require comment. I cannot refrain, however, from commending most heartily the method popularized by Kelly, of using repeated enemata of normal salt solution for their decided prophylactic as well as curative effects.

In my opinion the significance of this condition depends largely upon associated complications. When it exists alone it is transient, *rarely* causes systemic symptoms, and requires no treatment. When sapremia coexists the nephritis is intensified and the symptoms, due to retained poisons, often assume a serious aspect. They are promptly relieved by restoration of kidney activity. If septic infection occurs the nephritis is intensified and the system is less able to cope with the invading bacteria. The prognosis is rendered more grave, but depends upon the seat of infection as well as the number and virulence of organisms.

Summary. 1. Post-operative acute toxic hyperemia of the kidneys and nephritis are conditions that often exist, but seldom give rise to serious symptoms or permanent lesion. They occasionally cause both.

2. They are due to predisposing causes plus exposure during and after operation, and toxic agents such as anesthetics, ptomaïnes, leucomaïnes, etc.

3. Anesthetics are undoubtedly irritating to diseased kidneys, but often the condition requiring operation is such as to render a moderate kidney lesion a secondary matter. A hyperemia of the kidneys, and even a nephritis, is often relieved by appropriate operation, notwithstanding the effects of the anesthetic.

4. Anesthetics are also irritating to healthy kidneys, but in the vast majority of cases this effect is only recognized by a systematic study of urine before and after operation.

5. The bacterial poisons met with after operation are in themselves usually irritating to the kidneys, and when their effects follow immediately upon the congestion resulting from the anesthetic, impaired function results. The toxins along with effete products are retained.

6. The retained poisons give rise to their own definite train of symptoms, which differ widely from those usually met with in the ordinary forms of nephritis, and hence the absence of edema, coma, convulsions, and other classical symptoms.

7. This condition is analogous (with important differences) to the nephritis of the acute infectious fevers, in which the symptoms of the original disease are decidedly intensified with but a slight kidney lesion.

8. The practical lessons to be drawn are: (*a*) To see that the emunctories are acting normally and to use every effort to promote their continued activity. (*b*) To prevent undue exposure. (*c*) To use the smallest possible quantity of anesthetic. (*d*) To be absolutely clean in technique. (*e*) To recognize the condition at once and institute prompt and energetic treatment.

9. This condition has a significance of its own, because it occasionally causes serious symptoms, which may be averted or promptly relieved if recognized at once and appropriate treatment instituted.

In conclusion, I wish to express my thanks to Dr. J. D. Singley, pathologist to Mercy Hospital, for the privilege of reviewing

his records, and to the resident pathologists whose efficient services contributed much to the comfort and safety of the patients in question.

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THE ANTENATAL FACTOR IN GYNECOLOGY.

By JOHN W. BALLANTYNE, M.D.,
EDINBURGH.

It is now generally conceded that in the etiology of gynecologic affections there are two factors of paramount importance, the *traumatic* and the *infective* or toxic; but there is also a third factor, the *antenatal*, and to this perhaps too little heed has been given. Evident traumatic and infecting causes have overshadowed less evident predisposing causes; etiologic factors immediately preceding the resulting diseases have bulked more largely in the mind of the gynecologist than antenatal causes, which had their origin years before the uterus and ovaries awoke to functional life. Nevertheless, it is necessary for the full understanding of gynecologic problems that attention be paid to the antenatal factor.

TRAUMATISM AND INFECTION.

In cervical, vaginal, perineal, and vulvar lacerations everyone recognizes the traumatic factor. Year by year such lacerations have diminished in frequency as the direct result of improvements in the construction of obstetric instruments and of the growth of correct opinions as to their use. There has been in the last decade a noteworthy decrease in the number of cases calling for operation for repair of vesicovaginal fistulæ, and instances of grave laceration of the perineum are not so common.

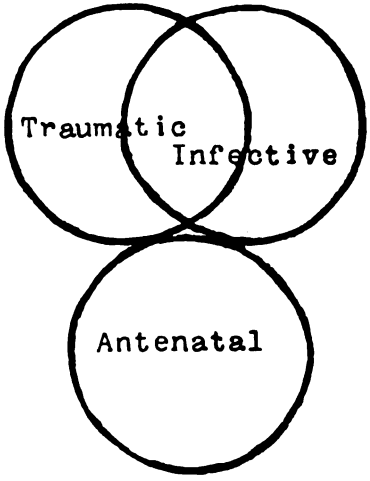
The great importance of the rôle of the infective factor in gynecologic etiology is now well established. Every text-book devoted to gynecology and every medical journal teems with allusions to the part played by sepsis, gonorrhea, and tubercle, in the production of inflammatory processes in the uterus, its adnexa, and in the vagina, vulva, and pelvic cellular and peritoneal tissues.

Uterine and adnexial displacements, and hypertrophic, atrophic, and hemorrhagic changes in the generative organs must in many instances be ascribed to this cause, acting either alone or in conjunction with traumatism. In this group are included not only the morbid states due to the action of micro-organisms, such as streptococci and gonococci, but also those caused by parasites such as echinococci and pediculi. A great part of the work of the gynecologist of the present day consists in the making of attempts, sometimes by medicinal means alone, but more often and more effectively by operative procedures, to undo the results of acute and chronic infective conditions of the genital organs. Most of the cases which he is constantly meeting can be traced in their origin either to immediate infection or to infection following after traumatism. Further, even in the cases in which operative interference is required for non-infective states, such as ovarian cystomata and uterine neoplasms, it is still infection, septic or otherwise, that the operator most dreads, and it is against infection that his best efforts are directed. Nevertheless, while all this is perfectly true, no gynecologist can be long in active practice without perceiving that traumatism, microbic and parasitic infection, and toxic influences do not serve to explain all the morbid conditions and all the phenomena connected with them, which he is every day encountering and having to treat. Ere long he suspects the existence of another factor; this is the antenatal.

THE ANTENATAL FACTOR.

By the antenatal factor in gynecology I mean more than the existence of gross malformations of the uterus and its adnexa, with their effects upon the performance of the functions of reproductive life. These, of course, are included; but I mean also all those abnormalities in structure, predispositions toward certain diseased processes, and inherited functional peculiarities which there is good reason to believe are determined antenatally, and which have oftentimes so powerful an effect upon the progress of gynecologic cases. The occurrence of such anomalies as atresia of the vagina, double uterus, and defective formation of the ovaries is well known to every gynecologist; everyone is able fairly accurately to forecast what the probable result of this or that malformation

will be. But there are other and more subtle ways in which conditions and tendencies, produced before the birth of the individual, project themselves into her later life; these are not so generally known, at least their far-reaching effects are not so fully appreciated. It may at once be admitted that it is not possible to arrange all the morbid states which affect the female generative organs under one or other of these three factors; an etiologic classification of gynecologic complaint is not so simple a matter. It is not practicable, for instance, to group together all the diseases of the uterus that are due to infection, and then all those that are due to traumatism, and then all those due to antenatal states in a linear series. It would be coming more nearly to the truth if the three factors were represented by three circles, two of which (the traumatic and the infective) bisected one another, while the third (the antenatal) touched the circumferences of the first and second, thus:



I do not forget that other causal factors than the three just named have been recognized in gynecology; they act chiefly through the nervous system and consist in unhygienic methods of education, in delayed marriage, in prevented conception, and in irrational modes of dress. Tight-lacing, however, might almost be placed among the traumatic causes; it is pressure which, if not dangerous at once, soon becomes so from its long continuance.

These errors, practised by one generation of women, become the antenatal causes of defective development of the whole system, and especially of the reproductive organs, of the next generation. I also admit the evil effects of alcohol and other drugs upon the generative functions of women and their progeny, but such causes I group with the infective and toxic agencies. With these limitations and explanations I think it may be accepted as true that most of the causes which lead to gynecologic complaints are either traumatic or infective and toxic or antenatal in their origin.

THE ANTENATAL FACTOR IN (A) THE MORBID ANATOMY OF GYNECOLOGY.

The antenatal factor is very evident in the morbid anatomy of gynecology. All the major malformations of the female generative organs and nearly all the minor ones are truly antenatal in origin. Trifling exceptions are found in the uterus pubescens, in atresia vulvæ superficialis arising from adhesive vulvitis in infancy, and in some hypertrophic conditions of the labia and clitoris. The various types of double uterus (didelphic, bicornate, septate), the uterus unicornis, the uterus rudimentarius, the uterus fetalis, the minor uterine malformations (incudiformis, parvicollis, etc.), and absence of the uterus; absence and atresia of the vagina, double vagina, unilateral vagina, and stenosis vaginæ; vulvar and hymeneal anomalies; absence and rudimentary development of the ovary, accessory ovaries, accessory tubal ostia and diverticula, and rudimentary tubes; and the various forms of pseudohermaphroditism; these are some of the admittedly antenatal morbid states of the female genitals. The morbid anatomy of all these anomalies is set forth at greater or less length in all text-books on gynecology (I have myself written the article on this subject for Allbutt and Playfair's *System of Gynecology*, 1896, pp. 63 to 112), and need not be entered into here. It may, however, be noted in passing that all these anomalies are arrestments of normal embryologic processes; they are the expression of the pathology of the genital organs during the stage of their evolution or construction; they represent morbid embryogenesis; and, judging by what is known of the causation of malformations of other parts of the body in the human subject and among animals, it may be presumed that the

disturbance of embryogenesis is brought about by the action of traumatism, microbes, or toxins upon the embryo *in utero*.

But antenatal diseases, as well as antenatal malformations of the female generative organs, are met with and leave their impress upon the later history of the individual in whom they occur. I have recorded several cases of fatal peritonitis, and in two of these there was displacement of the ovaries and Fallopian tubes of such a nature that, had the infants lived to the years of reproductive activity, they could hardly have escaped much suffering during menstruation and would probably have been sterile. Fetal pelvic peritonitis may also be instrumental in producing congenital or pathologic retroflexion or anteflexion of the uterus, with or without concomitant shortness of the vagina and conical cervix with pin-hole os; the far-reaching effects of these morbid conditions are well known to every gynecologist. Even prolapsus uteri, with or without hypertrophic elongation of the cervix, has been found so soon after birth as to prove that it existed potentially before birth. Two cases of this congenital form of prolapsus uteri were reported by J. Thomson and myself in 1897 (*American Journal of Obstetrics*, xxxv., 161); these were the seventh and eighth known examples of the anomaly, and since then four or five further cases have been recorded; and it is a striking fact that in all of them uterine displacement coexisted with spina bifida in the lumbosacral region. These occurrences suggest that perhaps some of the instances of prolapsus in the unmarried and in nulliparous married women may have an antenatal origin; and, bearing in mind the association of the prolapse with spina bifida, it will be well in future to examine cases of procidentia and descent of the uterus for spinal defects, and especially for spina bifida occulta.

Even the tumors which affect the female organs of generation may in some instances have an origin in antenatal life. This is especially true of the dermoid cysts or teratomata of the ovary. These growths are generally met in early reproductive life, even in some cases in childhood. Recent researches have revealed the existence of a long series of types of dermoid cysts, showing all the gradations from a growth containing only some hairs and skin to one containing a rudimentary but perfectly recognizable embryo. Their origin may be explained by regarding them as the result of fetal inclusion or enclavement or of parthenogenetic and imperfect

segmentation of ova in Graafian follicles; in any case the antenatal factor may be invoked. Further, many of the other neoplasms which render the operation of abdominal section so often necessary in modern gynecologic practice must be ascribed to the cystic degeneration of structures which existed in antenatal life and ought to have completely atrophied. I refer to paroöphoronic and parovarian cystomata.

THE ANTENATAL FACTOR IN (B) THE SYMPTOMATOLOGY OF GYNECOLOGY.

It is true that the symptoms that call attention to the maladies of the female generative organs are usually separated by a long interval of time from antenatal life, nevertheless they are not very rarely due to conditions developed before birth. The dysmenorrhea and sterility associated with congenital flexions of the uterus, and with defective developments of the Graafian follicles in the ovary from fetal pelvic peritonitis, may be justly ascribed to the antenatal factor. Similarly, dyspareunia and profuse hemorrhage during the first attempts at coitus are sometimes due to antenatal anomalies in structure or form of the hymen and external genitals. Amenorrhea, although most frequently due to physiologic conditions, is yet sometimes caused by such antenatal states as rudimentary development of the uterus, tubes, or ovaries, vaginal atresia, or hymeneal imperforation. In cases of amenorrhea in the unmarried, therefore, the physical examination of the genitals ought not to be too long postponed; for one or other of these congenital states may exist, and if this be so medical treatment need be no longer carried out and time thus wasted. Irregularities in menstruation may also be due to malformations, especially of the uterus; thus, in the double uterus, menstruation may occur every fortnight, every month, or once in two months. Fortnightly menstruation may be explained by the occurrence of a discharge from both uterine cavities once a month, but there is no coincidence of dates, and therefore there is a fourteen-day interval. Menstruation once in two months, again, may be due to a flow from one-half of a double uterus at intervals of two months, the other half of the uterus being imperfectly developed or imperforate. It is possible that the anomalous form of dysmenorrhea known as the *mid-pain*,

or *mittel-schmertz*, may be occasionally caused by uterine contractions in the imperforate half of a double uterus striving ineffectually to expel menstrual blood. Symptoms pointing apparently to disease of the nervous system may in certain cases be the result of congenital anomalies of the genital organs, such as adhesions of the clitoris, a condition resembling in many ways phimosis in the male. The history of the passage of feces from the vagina probably points, in the case of the multipara, at any rate, to the existence of the antenatal anomaly known as vulvar anus. Bleeding from the bladder at intervals of a month has been known to be due to vaginal atresia and the existence of a congenital communication between the uterus and the bladder. Examples might be multiplied, but sufficient instances have been cited to prove that even in the symptomatology of gynecology the antenatal factor must not be neglected.

THE ANTENATAL FACTOR IN (C) THE ETIOLOGY OF GYNECOLOGY.

I have already referred to the presence of an antenatal factor in the causation of the malformations of the uterus and the other organs of generation, of the so-called pathologic flexions of the uterus and displacements of the ovaries and tubes, and of the ovarian dermoids and parovarian and paroöphoronic cystomata; but there are yet other gynecologic morbid states of which the cause must be looked for in the life that precedes birth. For instance, extrauterine pregnancy has recently had two new theories advanced to explain its etiology, and both of these may be correctly described as antenatal. According to one, it is occasioned by the presence of an accessory tubal ostium abdominale or of a tubal diverticulum, and cases have been reported[1] of ectopic gestation in which these malformations were found. According to the other theory, the power to form a decidua is normally confined to the mucous membrane of the body of the uterus, but under certain circumstances this power may be possessed also by the mucosa of the Fallopian tube, for both the tube and the uterus are derived from the duct of Müller; it may be that through an arrest of the development of the tubal mucous membrane it retains this decidual

[1] Henrotin, F., and Herzog. Rev. de Gynéc. et de Chir. Abdom., 1898, ii., 633.

reaction. Even fibromyomata of the uterus have of late years come to be regarded as occasionally due in some measure to antenatal causes, and a very curious family history of the heredity of fibroids has been put on record by T. Spannochi (*Annali di ostetricia e ginecologia*, 1899, xxi., 331). There were three brothers called M., S., and P., and of these M. and S. married two sisters, A. and B. The descendants of M. and A. were free from abdominal tumors, but those of S. and B. showed in a very striking way the tendency to uterine fibroids and also to heart disease; there were nine children, of whom seven were females, and of the seven four had fibroids and two had also concomitant heart disease, while one of the three who had not fibroids had a daughter who developed a fibroid, and of the four daughters who suffered from fibroids one had three daughters, all of whom had fibroids and heart disease, and of these two were twins. The third brother, P., married E., a woman not related to A. and B.; there were five daughters and six sons from this marriage, in which it must be noted that the mother had no fibroid herself; two of the daughters had fibroids, while a third suffered from heart disease and gave birth to three daughters, of whom one suffered from a fibroid; further, one of the sons married and begat a daughter, who had both a fibroid tumor of the uterus and a cyst of the ovary, and she in her turn gave birth to three daughters, one of whom had already been operated upon for a uterine fibromyoma. In this remarkable family history not only does there seem to be an antenatal tendency to the production of female children with a predisposition to develop fibroids, but this tendency, curiously enough, seems to have been transmitted through the males in some instances. Of course, this antenatal transmitted tendency to produce fibroids is not incompatible with the theory of origin of such tumors from the muscular coat of the small uterine arteries or from proliferating congenital germs. In this relation reference may also be made to the curious family histories in which all the female offspring either developed cancer or were twins; and this is but another suggestion that cancer or the tendency to its development is prenatally predisposed to. Deciduoma malignum has also a very anomalous relation to antenatal life, for it would seem to be the result of abnormal developments not in the antenatal life of the woman who suffers from it, but in that of her progeny *in utero*.

THE ANTENATAL FACTOR IN (D) GYNECOLOGIC DIAGNOSIS.

There is not, perhaps, much need for me to insist upon the necessity that exists for the gynecologist to keep in mind, in forming a diagnosis, the possible presence of antenatal malformations of the genital organs. At the same time many of these malformations are so rare that even an experienced gynecologist may not have had the chance of seeing more than perhaps one or two of them in a lifetime. Further, the medical periodicals contain not infrequent records of errors in diagnosis which have arisen through the want of a just recognition of the possibilities of the antenatal factor. Thus, the abdomen has been opened for the removal of a uterine or ovarian tumor to find a pregnancy in the rudimentary half of a uterus bicornis ; fibroids of the uterus may be regarded as malformations of that organ, and more frequently malformations are mistaken for fibroids ; and atresia of the vagina leading to hematometra has been diagnosticated (through insufficient examination) as a normal pregnancy, and has led to unjust imputations upon the moral character of the girl who has been the subject of the vaginal anomaly. But, doubtless, the worst errors in diagnosis have been due to the non-recognition of male pseudohermaphrodites in early life. Through this mistake these individuals have been suffered to grow up wearing the dress and having the social position of females, and it has even happened to some of them to be married to men as women. The association of amenorrhea with the secondary sex characters of the male, in an individual apparently of the female sex, should always excite the suspicion of the gynecologist who may be consulted, and he ought to insist upon a physical examination of the patient. An interesting instance of this occurrence has recently been reported by Croom (*Trans. Edinb. Obst. Soc.*, 1899, xxiii., 102). It was the case of two individuals who had been brought up as sisters, but who turned out on examination to be really hypospadiac brothers ; fortunately, the mistake was discovered when the individuals were nineteen and twenty-one years of age respectively, and with the help of the lawyers new names were given to them and a new home and occupations were found for them in another part of the world. Manifestly, however, it would have been better for everyone concerned if the pseudohermaphroditism had been discovered at the time of birth.

In this relation it may be well to keep in mind that congenital ovarian hernia sometimes occurs and may be mistaken for an undescended testicle.

THE ANTENATAL FACTOR IN (E) GYNECOLOGIC PROGNOSIS.

The antenatal factor has occasionally intervened in somewhat curious fashion in gynecologic prognosis. Thus a case[1] was reported not very long ago in which an operator engaged in curetting a uterus thought that he felt the curette pass through the wall of the organ; in alarm he ceased his interference and awaited results with considerable fear, but no ill effects followed, and on a subsequent occasion he discovered that he had been dealing with a double uterus and that the curette had simply passed from one cavity of the viscus into the other, giving to the hand of the operator the sensation of perforation. The removal of the ovaries in order to induce a premature menopause in cases of uterine hemorrhage and in some kinds of nervous disease has not always been followed by the anticipated results, and it has been suggested that sometimes the error in prognosis has been the outcome of the existence of an accessory ovary or of a constricted piece of an ovary. Finally, it must not be forgotten that in gynecology, as in other departments of medicine, antenatal conditions have seldom so hopeful a prognosis as have the maladies which are developed during postnatal life; instances of this are forthcoming in the congenital displacements of the uterus, and in malformations of that organ and of the ovaries.

THE ANTENATAL FACTOR IN (F) GYNECOLOGIC THERAPEUTICS.

Considerable progress has been made in the rectification of the malformations of the genital organs which arise from antenatal causes. The operation for imperforate hymen may be described as perfected, and the treatment of atresia vulvæ superficialis may also be regarded as satisfactory. Further, recent improvements in the management of atresia vaginæ and of vulvar anus have been introduced, and it may be noted that the opening into the peritoneal

[1] Blondel. Ann. de Gynéc., 1898, vol. l., 137.

cavity, once so dreaded in the operation for the construction of an artificial vagina, is now rather the object aimed at than the contretempts avoided. At any rate, it is found to be advantageous to open into the pouch of Douglas in order to determine at once the condition of the uterus and ovaries. Into the whole question of the modern operative treatment of atresia vaginæ I have entered at length elsewhere (*Scot. Med. and Surgical Journal*, June, 1899). It must, however, be confessed that much still remains to be done in the reparative surgery of antenatal defects of the genital organs in women. Even in the management of the congenital flexions of the uterus and of the results of fetal peritonitis there is great room for improvement in present-day therapeutics. The management of pregnancy in the rudimentary horn of a bicornate uterus resolves itself into the management of ectopic gestation. The problem of the prevention of the malformations of the uterus and the other female organs of generation has scarcely yet been seriously investigated, for the sufficient reason that little has been known of the mode of origin of these anomalies. Of course, it has been recognized that arrests in the normal process of development of the ducts of Müller and of the mesonephros and the anlage of the ovaries explain the nature of most of the malformations, but in the absence of information concerning the causes of the arrests this knowledge avails little. Some light has of recent years been thrown upon the whole question of the cause of malformations and monstrosities, more especially by the methods of experimental teratogenesis. The general outcome of these experiments has been to strengthen us in the belief that the same causes which, when acting upon the adult organism, produce diseases, are effective in leading to malformations when they come into play before birth, and especially in the embryonic epoch of antenatal life. It is, therefore, to be expected that it will yet be shown that microbes and their toxins and toxic agencies, such as alcohol and lead and other poisons, and possibly also traumatism, are the ultimate causes of malformations.

It remains for future investigators to determine whether the anomalies of the generative organs, as well as of the other systems of the body, are more common in the descendants of parents who have been alcoholic, syphilitic, tubercular, or otherwise affected or unhealthy. Certainly, the most marked degree of malformation of the female genitals compatible with the postnatal life of the indi-

vidual that I have ever seen was in the person of a girl whose father died in an asylum and had been alcoholic, and whose mother was also a drunkard. In the face of such probabilities in the etiology of malformations, it will soon become a burning question what steps should be taken by medical men, by governments, by individuals to put a stop to or at least to limit the procreation of manifestly abnormal offspring. My views on this matter are contained in a lecture given in the University of Edinburgh in the spring of this year (1899) and published in the *British Medical Journal* for April 15th.

THE ANTENATAL FACTOR IN (G) GYNECOLOGIC JURISPRUDENCE.

I have already referred to questions in medical jurisprudence in which the antenatal factor plays a part, viz., the registration of the sex of pseudohermaphrodites; but there are several other questions besides those connected with individuals of doubtful sex which may come into the law courts and require an answer from the specialist in gynecology. I recollect, some years ago, when a certain *cause celèbre* was in everyone's mouth, hearing a well-known gynecologist state at a medical meeting that he was prepared to affirm that a woman who had a split or lacerated cervix must have been pregnant at one time or another. His assertion was, of course, challenged, and it was pointed out that a virgin or a nulliparous patient might have had her cervix artificially split in order to remove a fibroid tumor or intrauterine polypus. But it might further have been stated that laceration of the cervix might be present in a new-born infant as a congenital condition. Yet this is true, for of late years Penrose (*American Journ. Med. Sc.*, 1896, n.s., cxi., 503), Jefferson (*Med. Sentinel*, 1896, iv., 52), and Edwards (*Keating's Cyclopedia of the Diseases of Children*, 1899, v., 899) have all met with undoubted cases of congenital split of the cervix uteri with erosion. The condition is probably an abnormality in the arrangement of the mucous membrane of the cervical canal, a congenital histological ectropion. In addition to its purely medico-legal importance it may also be that congenital laceration of the cervix has some bearing upon the later development of cervical erosions in women, and even upon the origin of cancer of the cervix

uteri. One must take great care in the witness-box not to be too emphatic in stating what structural conditions may and what may not be compatible with chastity. As has already been shown in an earlier part of this communication, even prolapsus uteri, genuine and well-marked, may be met with in the infant a few hours old.

There is, then, a connection closer than might be expected between antenatal pathology and gynecology. There is a projection of the former into the latter, although many years may elapse before the results of the events which occur before birth are seen in the consulting-room of the gynecologist.

LAWSON TAIT.—1845–1899.

MEMORIAL ADDRESS ON THE LIFE AND CHARACTER OF LAWSON TAIT.

By CHARLES A. L. REED, M.D.,
CINCINNATI.

Death makes no conquest of this conqueror,
For now he lives in fame, though not in life.

Shakespeare.

It is eminently proper that a part of the proceedings of this meeting should be devoted to the memory of a deceased Honorary Fellow—Lawson Tait, who died at Llandudno, Wales, June 13, 1899.

This distinguished surgeon, the son of Archibald Campbell Tait, was born in Edinburgh, Scotland, May 1, 1845. His father was a guild brother of Heriot's Hospital, into which institution the son was admitted as a foundation scholar, at the early age of seven years. In this humble institution he early showed those traits of mind which enabled him to win a scholarship at the University of Edinburgh, and which characterized his subsequent brilliant career. We are informed that he did not graduate at this latter institution, although he studied in the departments of both the arts and medicine. Those who were students with him in this great institution remember him as a lad of great aptitude, but with comparatively little continuity of application. He was fond of those diversions which appeal with such force to vigorous young manhood. As a consequence, he found the prescribed tasks very irksome and the imposed discipline very annoying. He is reported as having been more intimately acquainted with the proctor than with any other officer of the institution. But his expanding and virile mind could be no less active than his vigorous body. The themes which engaged his attention, however, were those which occurred out of the ordinary routine. The great work of the Darwins was then creating a storm-centre in the scientific, to say nothing of the relig-

ious, world; and Huxley and Spencer were commanding alike the anathemas of the ecclesiastics and the plaudits of reactionary thinkers. It was to the latter class that young Tait belonged and continued to belong during his whole life. His devotion to the new philosophy brought him into antagonism with the orthodox thought that then dominated, as it yet dominates, this great seat of learning. It was in these controversies that he developed and displayed much of that polemic tendency and forensic power which in subsequent years proved at once his strength and his weakness. But the nature of the subject that constituted the basis of these early controversies was such as to direct attention to the more rational methods of study and investigation. Following the lead of Darwin he early learned to study things rather than words. There never was a time when he was not more attracted by an object itself than by the most artistic description of it. He early acquired the habit of translating words into ultimate conceptions—a habit which in subsequent years made him a severe and formidable critic for loose thinkers and careless writers.

It was precisely this preliminary training that fitted young Tait for the most successful prosecution of his medical studies. His pupilage in the medical school was in the essentially didactic era of medical teaching. Learned lectures prepared entirely with reference to doctrine and diction were the dominant features of instruction. The laboratories with their heartless iconoclasm scarcely yet had a footing. Personal authority was not yet dethroned. The dictum of the professor was yet accepted over and above demonstrated facts of contrary significance. Against this order of things young Tait, with great love for truth, but with no profound reverence for men, naturally found himself in revolt. In the medical school he was attracted and influenced by men who were already breaking away from the thraldom of tradition. Over and above the others of the class was the illustrious Sir James Y. Simpson, to whom the subject of our sketch sustained the relation of a special pupil and later that of assistant. He was also profoundly influenced by the teachings and the example of Syme, whose habits of cleanliness in his surgical work and whose results were in happy contrast with the methods and results of others connected with the Royal Infirmary. He was also an assistant to Sir Henry Littlejohn and took special instruction from Mackenzie Edwards. During

this time he gave particular attention to the various biologic studies, and particularly to investigations by the microscope, an instrument which, in the early sixties, did not occupy the prominence in scientific work that it does to-day. This early habit had a pronounced influence on his subsequent professional career, which really began at the early age of twenty-two.

After taking his L.R.C.P. and his L.R.C.S., he left Edinburgh, in 1867, to accept the position of house surgeon to Wakefield Hospital. It was here that he did his first ovariotomy, July 29, 1868, in the earlier months of his twenty-fourth year. He did five other ovariotomies between that and 1870, when he removed to Birmingham. Although he had done six ovariotomies by the time he was twenty-five, the fact does not seem to have turned his head in the direction of special work, for when he came to Birmingham he located in one of the suburbs and devoted himself to general practice. The year of his advent into the great midland metropolis was spent largely in preparing for and passing his English examinations. This year he took his M.R.C.S. Eng., and his F.R.C.S. Eng., and completed his Fellowship in the Royal College of Surgeons at Edinburgh.

At this time the young Scotch surgeon was very poor. It required but little persuasion to induce him to associate himself in office with Dr. Bell Fletcher, an established practitioner. He took this important step in 1871, and in doing so announced himself as a consulting surgeon. This in a young man of twenty-six required much courage, particularly in conservative England. Courage, however, was a quality in which Tait was never wanting. His self-assertion commanded recognition. The community began to estimate him as he estimated himself. He joined in a movement to establish a hospital for women—a movement which, in spite of the strong opposition of the conservative element, was crowned with success, and Mr. Tait was elected to the honorary staff, a position which he retained during the succeeding twenty-two years; after which he was made consulting surgeon, a distinction which he held at the time of his death. His identification with this movement was the real commencement of a career as illustrious as any in the annals of surgery. The succeeding year (1872) he performed two operations of historic importance. February 2d, he removed an ovary for suppurative disease, and on August 1st he

extirpated the uterine appendages to arrest the growth of a bleeding myoma. This simple statement sounds commonplace enough to-day, when the first of the procedures is exemplified daily in almost every operating-room in the world, and when the other, after a considerable vogue, has been supplanted largely by other and more satisfactory methods. It should be remembered, however, that in that day neither operation had been done, and that their performance was looked on by the time-servers as the unwarranted exploit of surgical audacity. Criticism began to flow in from every side. The chorus of fault-finding was augmented by those who sought to stifle his evolutional teachings at the Midland Institute, at which he was lecturing on physiology and biology. But he never failed to give a Roland for an Oliver—often, be it said, when the Oliver was not worth it. To Mr. Tait, however, criticism was not only a wholesome stimulus and antagonism, but an incentive to further endeavor. Fired by a realizing sense of the importance of his new achievements, and spurred on by the animadversions of his adversaries, he wrote his famous and epoch-making thesis on "Diseases of the Ovaries," for which the British Medical Association voted him the Hastings Gold Medal for 1873.

It was during this year that he did his first hysterectomy for myoma of the uterus, following, with but slight modification, the technique of Koeberlé. The next year—1874—he was instrumental in organizing the Birmingham Medical Institute, of which he became an original member. His activities of this sort, however, were not limited to the medical profession. He was interested in everything about him, but especially in art, the drama and politics. Despite his now rapidly developing work, despite the controversies to which it gave rise, and despite the antagonisms arising from his Darwinianism, which he continued to expound at the Institute, he sought further antagonisms by entering the political arena, although in a minor way. He was elected to the Town Council in 1876. While he was busy with all of these interests he was engaged also on work of a more enduring character. One was his now classic little work on *Hospital Mortality*, which appeared early the next year. It was really his final and crushing manifesto in the long argument he had been conducting with the conservatives. No work of equal size ever produced a more profound impression on the profession of England. It was followed the

same year by his little book on *Diseases of Women*. This small volume came to your speaker when he was practising his profession in an isolated village in a Western State. He had been taught to apply caustics, to replace the uterus, to adjust pessaries, and to do divers and sundry other things which, in the aggregate, made up the gynecology of that period. He had faithfully followed the teachings of the day—and had become convinced of their futility. The first ray of light came to him in the little book from Birmingham—and the darkness began to dissipate. Never was a book studied with more zeal—a zeal which a few years later led him across the Atlantic to Birmingham to see somewhat of the new surgery at the hand of him who had fashioned it. But it is to him rather than to myself that I must devote my discourse. Never was a surgeon or a citizen in his early thirties busier than was Mr. Tait at this particular period. The year that he published his *Hospital Mortality* and his *Diseases of Women* witnessed his adoption of the practice of removing the uterine appendages for incurable diseases of the Fallopian tubes. He removed a hematosalpinx June 21st, and made the profession familiar with its pathology. It was also about this time that the storm of antagonism against his Darwinianism broke out with renewed vigor, and ecclesiasticism exhausted itself in vituperation.

The next year—1878—witnessed his completion of a series of fifty ovariotomies, with nineteen deaths. This left him 13 per cent. for which to apologize, as the accepted inevitable mortality, as exemplified in the work of Sir Spencer Wells, was 25 per cent. But he was reasonably safe from criticism, for he had operated under the carbolic spray and in accordance with the canons of the then new Listerian gospel of surgery. Mr. Tait himself, however, was far from satisfied with either his results or the conditions under which he had secured them. He then and there expressed his doubts as to the efficiency of those procedures which in the aggregate were called "Listerism," but he at the same time seized on the central truth of the new surgery and proclaimed his belief in "scrupulous attention to cleanliness of every kind and in all directions." This was distinctly the beginning of "aseptic" as distinguished from "antiseptic" surgery; and I believe this audience will bear me out in the declaration that while each has its place, the former rather than the latter is to-day the accepted method of

procedure. At any rate, Mr. Tait, as was his wont, shaped his practice according to his convictions. He modified his technique in the direction of simplicity. One accessory after another was laid aside until a knife, scissors, needles, and thread, a few hemostatic forceps, some sponges and tap water were the essential constituents of his armamentarium. I shall never forget the contempt with which he alluded to the great parade of instruments in the operating-room of a distinguished Parisian surgeon, stating that it suggested an intention "to fight over again the battle of Waterloo."

It was with this simplified technique that he proceeded with his great work. It was during this year that he first opened the abdomen for the treatment of pelvic abscess. The next year—1879—fairly bristled with new achievements. He did his first cholecystotomy, and his explanation and defence of the operation marked the beginning of the rational surgery of the gall tract. This same year he removed the first pyosalpinx and the first hydrosalpinx in his practice, and again called attention to pathologic conditions that had been described a half century before by more than one writer. So strong had become the opposition to him by this time that it was quite fashionable, especially in London, to openly discredit everything that he said or did. A most distinguished metropolitan operator at this time openly expressed his doubts, not only as to the fact of Mr. Tait's newly-heralded operation, but also as to the very existence of the disease for which it was alleged to have been done. Mr. Tait, however, answered the criticism by his exhibiting his specimens—and went on with his work. Within this busy twelvemonth he described his flap-splitting operation for repair of the perineum, introduced his method for the reposition of the inverted uterus, developed his plan for the dilatation of the cervix by continuous elastic pressure—and not satisfied with this, he organized the Birmingham Natural History Society, publishing a valuable paper on the minute structure of the pitcher plants! He was also interesting himself in archeology, and published at different times articles on "Orientation of Churches," "Prehistoric Fortifications," "Monumental Brasses," and "Britain During the Stone Age."

The next decade opened on this resourceful man in the very maximum of vigor. His first work in the eighties was to introduce and establish the operation of hepatotomy—his cases rapidly

LAWSON TAIT IN 1884.

multiplying until he was able to publish a series of ten, nine for hydatids and one for abscess. This report opened the eyes of the surgical world, but not more so than did another that he published during this year. This was his second series of fifty ovariotomies with only three deaths as against nineteen in his previous fifty. A drop in his own mortality from 38 to 6 per cent., a point 19 per cent. lower than that of Sir Spencer Wells, was an evidence of achievement that required no wordy proclamation. What was more, these results had been obtained by Taitonian rather than Listerian methods—by "aseptic" rather than "antiseptic" formula. This was the first strong manifesto of Mr. Tait against Listerism, and was the real beginning of an antagonism that he waged until his death.

The next two years—1881–82—were not so fruitful in intellectual results as the dozen which had preceded. It was in 1881 that Mr. Tait permitted an opportunity to pass unimproved, whereby a patient lost her life and science was for a time deprived of a helpful impetus. He was called by Mr. Hall-Wright and urged to operate for a case of ruptured tubal pregnancy. He hesitated, and the patient died. The autopsy showed the feasibility of the operation. I shall ever remember the description of his chagrin as Mr. Tait told me of this incident several years later. It was not until January 17, 1883, that he encountered his next case of this character. It may be safely surmised that there was no hesitancy this time. He operated and saved his patient. A series of thirty-five cases of operation for ruptured tubal pregnancy with but two deaths speedily followed, and its publication put this surgical resource on a sound footing. While all this and other work was in progress, this indefatigable man found time to expand his Hastings Prize Essay into a treatise on *Diseases of the Ovaries*, and to see it through the press.

In 1884, Mr. Tait, prompted by a catholicity of spirit which protested against what he termed the exclusiveness of an existing London organization of which he was a member, joined a number of his friends in the organization of the British Gynecological Society, of which he was elected vice-president. During this same year he published his first recorded 1000 cases of abdominal section. This was followed the next year by the publication of his startling record of 139 consecutive ovariotomies without a death. This

remarkable record was also offered as a protest against Listerism. It was assailed, as a matter of course, and the question became one of veracity. But the cases were of record, and, when I visited him a few months later, I had the satisfaction of examining the bedside notes, all duly signed by those present at the operation and by the nurses who kept them. With all the protesting, not an allegation in that record stands disproven. It was in 1884 also that Mr. Tait visited America at the invitation of one of our Fellows, Dr. Vander Veer, and demonstrated his operations in the hospitals of New York, Albany and other cities.

One would suppose that so much of professional work would exhaust all of at least one man's energy, but such was not the case with this remarkable man. About this time he withdrew from the Town Council and contested, unsuccessfully, the Bordesley division as a radical home-rule candidate for Parliament. He was also a writer of leaders for the principal newspaper of his party in Birmingham, in which enterprise he also had a pecuniary interest. He was also a shareholder in a theatre, and gave some personal attention to its conduct. In 1886 he was the president of the British Gynecological Society and went regularly to London to preside over its monthly meetings. With all of this activity he seemed to crave still further responsibilities. In 1888 he delivered the Ingleby lecture, choosing "Ectopic Pregnancy" as his topic. The same year he accepted the professorship of gynecology in Queen's College. One of the earliest fruits of his professional labors was the collection of his previous writings and their publication the following year in the first volume of a projected two-volume work on *Diseases of Women and Abdominal Surgery*—the second volume of which never appeared. This same year—1889—he was elected president of the Birmingham Medical Institute, an office which he held during the next four years. But it is impossible, in the brief space of an address such as this, to trace, step by step, the great work accomplished by this man. To the brief *résumé* which I have already given I must add that he was elected to the presidency of Mason College, and that he was awarded the Cullen and Liston Memorial Prize by the Edinburgh College of Physicians for his services to medical science. From America, also, he received honorable recognition. The University of the State of New York conferred on him, *honoris causa*, the

title of Doctor of Medicine, as did the St. Louis College of Physicians and Surgeons, and he was an Honorary Fellow of both the American Gynecological Society and of the American Association of Obstetricians and Gynecologists.

The last five years of Mr. Tait's life were marked with a more or less continuous invalidism. He was operated on in London for stone, and subsequently developed a chronic nephritis. These and other depressing influences prompted him largely to relinquish his operative work, and he sought repose at beautiful Llandudno, facing the Conway estuary and commanding a view of the Penmaenmawr Mountains. So impressed was he with the sanative features of this location that, but a short time before his death, he purchased an inn on a high point of land, with the object of converting it into a sanatorium for consumptives. But he was not permitted to carry this philanthropic plan to fruition, as he was suddenly seized, June 3d, with renal complications, from which he succumbed, June 13, 1899. In accordance with his request his remains were cremated at Liverpool. In further compliance with his wishes his ashes were deposited in a cave on his private grounds.

It is difficult to summarize a life of such originality and activity. It may be said of Mr. Tait, however, that he laid the foundation of modern abdominal and pelvic surgery; that he personally devised the chief operations that pertain to that department of practice; that he invented many new instruments and improved many others; that he refined surgical technique to its present simplicity; that he demonstrated for the first time much of the now accepted pathology of the uterine appendages; that he reduced the mortality in abdominal and pelvic surgery to the minimum; that he, more than any other one man, forced the reforms whereby modern hospitals have been brought to their present high state of efficiency; and, finally, that he has added many years of life and happiness not only to women but to mankind. Living *primus inter pares* his death leaves the largest possible vacancy in the productive intellectuality of the surgical world. But the influences he set in motion are happily immutable, and illustrate, in the words of Longfellow, that

> —when a great man dies,
> For years beyond our ken,
> The light he leaves behind him lies
> Upon the paths of men.

My own personal acquaintance with this great man began in the winter of 1886. In the midsummer of that year I had published a short series of abdominal sections which I had done as nearly as I knew how in accordance with his teachings. The report, although published in a local American journal,[1] attracted his attention, just as did every other report of the kind published at that time in any part of the world. It was said of him at this time that he saw every sparrow that fell from the gynecologic heavens. He at once sent an appreciative letter to the journal and another to myself, inviting me to visit him. His letter found me in the final acts of departure for the voyage. The promptness with which I responded to his invitation must have been a trifle startling. On my way to New York, however, I was told that "Tait had said that he did not want to be bothered with any more American tramps." This rankled me not a little, and the more so the nearer I got to England. When I reached Queenstown I sent a message to a personal friend in Birmingham asking him to inquire into the correctness of the report, and to advise me at Liverpool, for I had made up my mind to never cross the threshold of one who had spoken thus opprobriously of my compatriots. I was advised to come on, that "it was all right," and my reception was indeed most cordial. This man, who was startling the professional world, and the magnetism of whose genius had lured me across the Atlantic, was indeed a striking personality. Of rather less than medium stature, broad-shouldered and deep-chested, he carried, in splendid poise, a head to describe which one can think of no other word than leonine. His full growth of very dark hair was in striking contrast to the prevailing fashion, while his closely cropped side whiskers gave him a distinctly English expression. His eyes, scintillating with intelligence, were now penetrating one with their attentiveness, dancing merrily at some pleasantry, flashing defiance at the narration of some wrong, and, anon, expressing tenderest sympathy over some picture of human suffering. His well-chiselled mouth was another of his most expressive features, and by its mobility could be seen the play of emotion that engaged his mind and heart. It was a royal feast, attended only by Mr. Tait, his charming and faithful wife, United States Consul Hughes, and myself. There

[1] Columbus Medical Journal, July, 1886.

was a cordiality in the atmosphere that made one feel at home. All the genial sympathy was enhanced by the captivating frankness with which the host discussed the personal aspects of his great struggle. "No," said he, "far from disliking the Americans, I am really fond of them. I owe them a debt of gratitude that I cannot easily repay, for if it had not been for the recognition and support they gave me, and are yet giving me, I could not have succeeded in my fight against English conservatism." He repeatedly made this declaration to me, and he reiterated it in a letter which I received from him but a few years ago, and in which he delineated the rise and development of "The Birmingham School." I mention this only to show Mr. Tait's real sentiments toward America and Americans. While I do this, I do not wish to conceal the fact that he had grievances against certain of our countrymen, and that he realized it. This grievance was against those who, while spending months at other hospitals, would run up to Birmingham for a day "just to see Tait operate." The material at Birmingham was always so abundant that they were generally gratified. They saw a simplicity that was strikingly in contrast with the complex procedures they had seen elsewhere, and they saw a celerity of operation that, in contrast with the usual slowness of uncertain workers, looked much like recklessness. They tarried only to satisfy their curiosity, and went away without knowing the man or understanding his methods or familiarizing themselves with his results. These people generally constituted themselves his public critics, and printed animadversions that were generally colored by the London prejudice to which they had willingly subjected themselves. If Mr. Tait did not call these people "tramps" he ought to have done so, for no word in our language so aptly defined their leading characteristics.

The weeks that followed my initial interview were replete with valuable experience, delightful personal association and cordial hospitality, so that when my leaving came I felt that I was leaving the home beneath whose roof I had imbibed the inspiration for my life-work.

The surgeon who thus imbued me with admiration and confidence was the most dexterous and resourceful whom it has ever been my privilege to observe. He did nothing with the appearance of haste, but moving by direct lines and to a definite purpose, every

crook of his finger counted. No complication seemed to dismay him, and no emergency disturbed his equanimity. His dexterity was phenomenal, and his celerity a marvel, even in a country that abounds in operators of pre-eminent ability. I have seen him begin and complete an ovariotomy inside of five minutes, and conclude a hysterectomy within twelve minutes after making the initial incision. While this was true, I have seen him spend an hour over an open abdomen when complications and the welfare of his patient demanded it.

The man who thus impressed me had indeed a remarkable personality and one difficult to delineate. His manners were of the vacillating kind. At one time he would be brusque and inconsiderate, forgetting his mood the next minute in an exhibition of deferential courtesy that was captivating. If it cannot be said of him that he always observed the Emersonian dictum "that manners are made up of petty sacrifices," he often demonstrated how politeness could embrace generous benefactions. As a host he was captivating in his hospitality. Himself fond of the pleasures of the table, he encouraged his guests to similar indulgences. As a story-teller he had but few equals, and his *repertoire* was full of the latest novelties, many of them of his own invention. I never heard him tell a story malapropos. He was a veritable flash-light in repartee, and, to cling to my metaphor, often blinding in his merciless brilliancy.

As a public speaker he was clear and incisive, never pausing for a word, a habit affected by the average parliamentary speaker, but always selected the right one for the right place. He was magnetic, and his discourses were more frequently interrupted by applause than were those of many successful speakers. So accurate were his extemporaneous utterances that they could have been safely published without revision. He always spoke to a purpose, and was never a "tuner of accents." As a writer he was especially able on controversial themes. While his language in his descriptive works was always correct, and his diction elegant, he lacked completeness, system and arrangement. These defects made it difficult to utilize many of his writings as working formulæ, although everything that he wrote contained a germ of truth that sprouted and grew to rich fruition in the fertile soil of the profession.

It is impossible for me to portray the inner character of him the incidents and surface indications of whose life I have briefly summarized. There is that in the life of men that defies scrutiny and eludes the pen. Who shall weigh courage, or measure tenderness, or estimate love? Never were elements better combined to defy the cunning of analysis. It were easy and natural in speaking of Mr. Tait to use such words as able, energetic, aggressive, inventive, resourceful, brave and tender, but it is not easy to portray the psychic play whereby these and other qualities were blended in varying degree to produce the character that he revealed under varying circumstances. We can only follow the advice of Locke, who said that the "actions of men are the best interpreters of their thoughts." To interpret one's thoughts is to interpret one's character. If we are to interpret Mr. Tait's character by his actions, we are at once impressed that he was a man of great character, for he was a man of great action. With him activity seemed to be as essential as breathing is to his existence. It was with him veritably a question of do or die. He acted openly before the world, and invited the closest scrutiny. He realized that, fundamentally, all action relating to the common good is, and of right ought to be, public. He was always ready for the task at hand, and thus went naturally from one achievement to another. Thus opportunities which at first seemed remote became present activities. Sophocles said: "Heaven never helps the man who will not act." The activities of Mr. Tait certainly entitled him to large benefactions from Heaven. One of the earliest of these benefactions came to him in the guise of adversity. He felt its pangs, but with his propensities it was, indeed, the severe mistress that forced him to high endeavor. It freed him to believe that what man had done man could do, and to realize that he was a man.

He set himself to high things—and he speedily justified his presumption. Adversity rarely travels except under the escort of enmity. To no man have enemies been of more value than to Mr. Tait. Rather than to have been without them he might well have purchased them with pure gold. They aroused him to activity, and developed the best that was in him. He answered them openly, frankly, often severely. While he was always frank in acknowledging their merits, he never offended good taste by treating them to "a diet of praises sauc'd with lies." Carlyle justly

observed that "no man is born without ambitious worldly desires," and Mr. Tait had his full share of ambition. He was indeed an exemplification of the fact that "ambition has no rest." He was anxious to place his name on the scroll; he was proud of his achievements, and he was interested that the record should be written with truth and justice. He seemed daily to illustrate the declaration of Scott that

> One crowded hour of glorious life
> Is worth an age without a name.

He was distinctly the man for the time. At his advent professional opinion, like a dear old lady who had feasted well, was drowsing in an arm-chair. He heralded new things in thunder tones, poked the drowsy jade in the ribs and broke up the siesta. Of course, there were angry protests, but they availed nothing, for he went on thundering and prodding until everybody was awakened. This was his life. In its busy course he often paused to smooth the troubled brow, to feed the hungry, and to give to poverty wherewith to cover its nakedness. He loved trees and flowers, and heard music alike in rustling leaves and gurgling brooks. His heart of tenderness won him to the side of defenceless brutes, and he became their champion in the ceaseless conflict between sentiment and reason. Withal, he had wisdom, for oft he knew that he did not know, and, not knowing, sought to know. No adversary ever criticised him half so severely as he criticised himself. Often impulsive, he was yet given to deliberation, and in such deep thought is wisdom born. His wisdom, however, manifested itself in firmness of mind rather than in mastery of appetite. Indeed, he, like many of the most illustrious in history, may have had occasion to exclaim with his own beloved Burns, that "Nature formed me

> With passions wild and strong,
> And list'ning to their witching voice
> Has often led me wrong."

If this were true it was because he lived intensely, and, living thus, he lived, not long, but much. While he lived he lived in deeds, thoughts, feelings, and heart-throbs, taking little account of years or clock-dials. In the intensity of his life he erected a monu-

ment of majestic good, which alone appeals to memory, and beneath which mote and blemish have sought their own oblivion.

I prefer to remember Mr. Tait as I last saw him. It was four years ago, during the session of the British Medical Association in London. One morning, at his suggestion, we slipped away from the meeting, and, joining my family, we spent the day on the Thames. We were overjoyed to have him thus to ourselves. Although he was showing some signs of a recent illness—possibly some traces of the deep malady that finally carried him off—he was strikingly interesting in his comments on the old landmarks that throng either bank of that historic river. History, anecdote, criticism, and comment flowed from him in a sparkling stream, while at times he joined in the merriment of the children with the zest of youth itself. We returned to London late at night. I escorted him to the National Liberal Club, where he was living. He insisted that I tarry with him a while, and conducted me to a little corner balcony overlooking the great reading-room. "Stand there with uncovered head," said he. "It's as hallowed a place as there is in all England!" It was the vantage-point from which the immortal Gladstone was wont to address his faithful followers. He then conducted me up to the tower, from which the great panorama of London lights was visible. It was an impressive sight. As London typifies the world, so did this revelation of it seem to inspire him with world-wide themes. He was away from the hospital, away from the chamber of sickness, from the hall of controversy, and, thus unchained, his great mind revelled in its freedom. England, the Indies, Africa, Anglo-American relations, the then recent war between China and Japan, the contrasting features of Occidental and Oriental civilizations, the great ethnic movements of the world, music, the drama, human happiness, and life itself were themes that he touched with the spark of illumination. As we talked, "Big Ben," in Parliament Tower hard by, tolled twelve, anon one, and still anon two. The moon rose, and the silhouette of the great city was seen against the tinted sky of the East. The speaker of great thoughts paused to view the majestic scene. I receded a step or two, that I might contemplate, in clearer perspective, the more impressive picture of triumphant genius with the sleeping world at his feet! Thus may he abide in peaceful memory!

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